

SCIENCE, TECHNOLOGY & ECONOMIC GROWTH IN THE EIGHTEENTH CENTURY

Edited by
A. E. Musson

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HISTORY AND PHILOSOPHY OF SCIENCE

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Science, Technology, and Economic Growth in the Eighteenth Century

edited with an introduction by
A. E. MUSSON

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Preface

This volume in the *Debates in Economic History* series is concerned with one of the important, though less publicized, of recent debates in economic history: the role of science in technical change during the Industrial Revolution. One may predict that much more attention will be given to it over the next few years as research into the springs of technical change and the diffusion of technology continues. It has all the attributes of the best historical controversies. Both the Scientific Revolution and the Industrial Revolution are clearly of the very first historical importance and intrinsically fascinating. Both occurred in Western Europe, uniquely so in the history of mankind, in close juxtaposition in time. Each is also of such a degree of generality, sufficiently unspecified conceptually, with such a complex of variables – most of them unquantifiable – interacting in such a bewildering variety of ways, that the debate about how they are causally related is, in the final analysis, unresolvable. Because ‘scientific’ proof of causation is thus impossible, all participants in the debate can rest happy in the awareness that their contributions, whether of more data, new hypotheses or judicious trimming between old ones, cannot be absolutely discounted as provably false or demonstrably irrelevant.

A controversy in such a happy state flourishes as the generations pass, bringing new evidence as well as new ideas to the traditional stock-pot. In the present case new research, not least that of Mr Musson and Dr Robinson, is much enlarging the awareness of economic historians about the extent of scientific interests among eighteenth-century entrepreneurs and innovators. In no field of common interest are there larger gains to be made by the collaborative efforts of economic historians and historians of science. For a general editor who finds himself caught in the debate as a contributor, that provides a discreet note upon which to end a preface.

All Souls College, Oxford
31 August 1971

PETER MATHIAS

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Editor's Introduction

I

The study of the relationship between science, technology, and economic growth during the Industrial Revolution necessarily involves consideration and attempted integration of a vast amount of work, both empirical and theoretical, in different academic disciplines. The greatest quantity of factual evidence has been collected by economic historians, though a good deal of research has also been carried out by scientists, engineers, and others interested in scientific, technological, and industrial history. At the same time, current interest in economic development and growth has made such studies of closer relevance to the theoretical work of economists and sociologists. It is therefore the main purpose of this introductory chapter to trace the evolution of economic and sociological theories in regard to the problems presented by scientific and technological progress, and against this theoretical background to view recent historical studies on these aspects of the Industrial Revolution.

The scientific and technological achievements of the past two centuries are so overwhelmingly obvious in their transformation of economic and social life that it seems almost incredible that, until very recently, most modern economists, building theoretical growth 'models', left them entirely out of account. These immense forces of change – revolutionizing industrial organization and production, prodigiously expanding trade and transport, requiring vast amounts of capital, and altering the whole structure of the labour force and society in general – have been regarded as 'exogenous' or external to the economic system! Economic historians, on the other hand, have always given great prominence to industrial-technological developments: inventors and entrepreneurs crowd the stage in economic history, together with the workers and population generally whose lives have been so profoundly affected by their innovations. Economic history has retained a much greater realism, a much broader approach, and, on account of the multiplicity

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and complexity of the factors involved, has tended to be strongly empirical, utilizing only a loose framework of theoretical ideas. But now the resurgence of interest in business studies and technological growth among economists raises the possibility of closer collaboration with economic historians, who, on their side, are now recognizing more clearly the uses of theoretical and statistical tools in historical analysis.

The neglect of scientific and technological developments by economists became pronounced only in the late nineteenth century, when economics tended to become increasingly academic and theoretical, narrower and more remote from industrial and commercial reality than earlier economic writings, in which there was a much broader approach.¹ Mercantilists, for instance, in the seventeenth and eighteenth centuries,

stressed the importance of invention and technological improvements and the need to introduce them speedily and effectively into industry . . . by encouraging the development of empirical science and its application, not only to the solution of military problems, but also to the solution of mining, transport, and other problems of economic import. . . . The practical application of science received continuing stimulus from mercantilist Francis Bacon's endorsement of innovation and gradual change and from his philosophy of utility. . . . Because the importance of invention was recognized by many English mercantilists (e.g. Grew and Petty), some under the influence of Bacon, by French mercantilists (especially Colbert), and by various others, they looked with

¹ There is a vast literature on theories of economic development from Adam Smith onwards. See, for example, G. M. Meier and R. E. Baldwin, *Economic Development: Theory, History, Policy* (New York, 1957); B. Higgins, *Economic Development: Principles, Problems, and Policies* (New York, 1959; new ed. 1968); B. F. Hoselitz *et al.*, *Theories of Economic Growth* (Glencoe, Illinois, 1960); I. Adelman, *Theories of Economic Growth and Development* (Stanford, California, 1961); Y. S. Brenner, *Theories of Economic Development and Growth* (1966). See also general histories of economic thought, such as J. A. Schumpeter, *History of Economic Analysis* (New York, 1954); R. Lekachman, *A History of Economic Ideas* (New York, 1959); O. H. Taylor, *A History of Economic Thought* (New York, 1960); M. Blaug, *Economic Theory in Retrospect* (1962; 2nd ed. 1968); W. J. Barber, *A History of Economic Thought* (New York, 1968).

favour upon the establishment of academies and schools to promote and diffuse science and its application.¹

The later classical economists, while generally attacking mercantilist restrictions and advocating *laissez-faire* and free trade, showed a similarly broad concern with industrial-technological and social as well as more narrowly 'economic' factors.² Adam Smith's *Wealth of Nations* (1776) shows a strong awareness of the dynamic forces behind economic growth. In particular, he emphasized the importance of division and specialization of labour, resulting from expanding markets and leading to improvement in skills, technology, and productivity; he also stressed social factors such as growth of population, changing consumer wants, and ambitions of merchants and manufacturers to acquire wealth, prestige, and power. Moreover, Smith pointed out that improvements were introduced not only by practical craftsmen but also by 'philosophers, or men of speculation', and he remarked how with the growth of trade and specialization 'the quantity of Science is considerably increased'.³ This observation, according to Professors Carter and Williams, shows how much he was 'ahead of his time';⁴ but these authors, while notable among present-day economists for their emphasis on the importance of applied science to modern industrial growth,⁵ have repeated uncritically the traditional view of the Industrial Revolution as almost entirely the product of uneducated empiricism, though they make an exception of James Watt and the steam engine. In fact, however, Smith was simply observing the developments of his own day,⁶ although, of course, it is true to say that at the time when

¹ J. J. Spengler, 'Mercantilist and Physiocratic Growth Theory', in Hoselitz, *op. cit.*, pp. 46-8. To Bacon, Grew, and Petty we can add Hartlib, Evelyn, and others who similarly emphasized the importance of applied science in industrial development.

² J. M. Letiche, 'Adam Smith and David Ricardo on Economic Growth', *ibid.*, pp. 65-88, and E. McKinley, 'The Theory of Economic Growth in the English Classical School', *ibid.*, pp. 89-112; B. E. Supple (ed.), *The Experience of Economic Growth* (New York, 1963), pp. 12-14.

³ *Wealth of Nations*, book 1, chap. 1.

⁴ C. F. Carter and B. R. Williams, *Investment in Innovation* (1959), pp. 149-50.

⁵ In addition to the above-mentioned work, see their *Industry and Technical Progress* (1957) and *Science and Industry* (1959).

⁶ A. E. Musson and E. Robinson, *Science and Technology in the Industrial Revolution* (Manchester, 1969), p. 59 *et passim*.

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he was writing, the Industrial Revolution had not gone far, and that he therefore 'wrote more about pin factories than about iron fabrication',¹ or other technological changes then in their infancy.

Smith's successors in the English classical school, living in the midst of the Industrial Revolution, were also very much concerned with the growth process, though they tended to be pessimistic about long-term prospects, envisaging ultimately a stationary state, as a consequence of scarcity of natural resources, population pressure, falling profits, etc.; they underestimated the future flow of technological improvements and investment possibilities. It must be remembered, however, that they were writing amid the uncertainties of revolution, war, and their aftermath, causing profound economic and social upheavals. But Ricardo did observe that British manufacturing superiority was brought about 'by the improvements in machinery, by the better division and distribution of labour, and by the increasing skill, both in science and art, of the producers'.² With much greater emphasis, John Stuart Mill included among the principal forces responsible for increased production the progress of science and technology, education, and entrepreneurial enterprise, as well as capital formation, population growth, division of labour and improved skills; education, indeed, he regarded as the most dynamic factor, both for its general socio-economic stimulus and for its contribution to the development and diffusion of scientific-technical knowledge. He has been described, in fact, as 'the first British economist to stress the importance of applied knowledge'.³

The neo-classical economists of the late nineteenth and early twentieth centuries, however, including Jevons, Edgeworth, Marshall, Wicksteed, Menger, Walras, Pareto, Clark, Fisher, Wicksell, and Pigou, turned their attention away from long-term growth to short-run functioning of the market mechan-

¹ Barber, *op. cit.*, p. 24.

² *The Works and Correspondence of David Ricardo*, ed. P. Sraffa (Cambridge, 1951-5), vol. I, p. 94.

³ J. J. Spengler, 'John Stuart Mill on Economic Development', in Hoselitz, *op. cit.*, pp. 113-54.

ism – from ‘dynamic’ to ‘static’ analysis.¹ This also involved a shift away from the study of large aggregates to the examination of the detailed working of the economic system – the decisions of producers and consumers, interacting to determine market prices – so ‘macro-economics’ was mostly abandoned for ‘micro-economics’. Thus was developed the theory of the margin and general equilibrium. More narrowly academic than their predecessors, and impressed by the methods of the physical sciences, the neo-classical economists discarded the empirical socio-historical approach and adopted more rigorously theoretical and mathematical techniques. ‘Neo-classical authors . . . replaced real-world firms and households for analytical purposes by relationships couched in mathematical language, e.g. by production functions and utility functions.’² Simulating scientific laboratory techniques, they constructed abstract models, from which they sought to eliminate uncertain, random, or disequilibrating variables. They not only made the unrealistic assumption of ‘perfect competition’, but also focused their attention on a narrow range of factors in a static situation, eliminating time and long-term variables from their analysis (*ceteris paribus*), especially those of an ‘exogenous’ or ‘non-economic’ character – among which, of course, were science and technology, population, natural resources, etc.³ This concentration on static analysis of limited problems was not, as might perhaps be thought, through any recognition of their inability to deal with the complexities of long-term development; their writings indicate, in fact, an assumption that, under the beneficent influence of free competition, economic growth would take care of itself – that it was not really a problem at all – a complete change in attitude and interests from those of the classical economists. This was probably a reflection of the contemporary optimism and faith in progress,

¹ J. Buttrick, ‘Toward a Theory of Economic Growth: the Neo-classical Contribution’, *ibid.*, pp. 155–92. See also above, p. 2 n. 1.

² H. J. Bruton, ‘Contemporary Theorizing on Economic Growth’, *ibid.*, pp. 239–98. Some, such as Walras, were more ‘purely’ theoretical, mathematical, and ‘scientific’ in their approach than others, such as Marshall, who were closer to the realities of life.

³ It is true that Marshall, for example, did include both ‘short-run’ and ‘long-run’ in his analysis, but these vague time-periods were not historical in scale. His treatment of ‘the secular period’ was comparatively brief and inadequate.

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resulting from a century of continued and unprecedented economic expansion.

It is true that all economic theory during this period was not characterized by static, micro-economic methods. The German Historical School of economists, including List, Hildebrand, Bücher, Schmoller, and Sombart, emphasized long-term factors and developed various theories of 'stages' of economic growth.¹ These were variously concerned with changes from primitive to modern societies, from barter to money and credit, from agricultural to manufacturing and commercial activities, from village to urban to national economic organization. They attached considerable importance to social and technological, as well as economic factors,² but were generally weak in their explanations of the dynamics of change. Marx, of course, similarly formulated a stages theory – developing from primitive society, through feudalism, to capitalism, and finally to socialism – in which basic changes in technology and productive organization determine class relations and other social, political, and cultural parts of the superstructure.

These historical theories, however, were never accepted into the general body of economic analysis during the neo-classical period. With the exception of Marx, and one or two individual theorists, it has been stated, 'for almost a century after 1850 there was no fresh systematic discussion of the nature of economic development'.³ This is perhaps going too far: even in the neo-classical period, economists such as Marshall wrote broadly on the development of industry and trade, and his theory did contain some 'dynamic' or evolutionary elements

¹ B. F. Hoselitz, 'Theories of Stages of Economic Growth', in Hoselitz, *op. cit.*, pp. 193–238, and Brenner, *op. cit.*, pp. 150–74.

² We find Sombart, for example, writing of the growth of 'the capitalist spirit' in the early modern period: 'The same spirit out of which was born the new state and the new religion, the new science and the new technology, also created the new economy.' W. Sombart, *Der Moderne Kapitalismus* (1921), vol. I, p. 327. But he considered that seventeenth-century technology was essentially empirical, almost completely divorced from contemporary science (*ibid.*, pp. 466–7).

³ Supple, *op. cit.*, p. 14. See also W. A. Lewis, *The Theory of Economic Growth* (1955), Preface, where it is stated that 'no comprehensive treatise on the subject has been published for about a century. The last great book covering this wide range was John Stuart Mill's *Principles of Political Economy*, published in 1848.' See also S. Kuznets, *Economic Growth and Structure* (New York, 1965), pp. 4–5.

(he recognized, for example, the effects of technological progress); Juglar, Tugan-Baranowski, Spiethoff, Robertson, Pigou, Kondratieff, Kitchin, and others studied industrial fluctuations; while historically-minded economists such as Chapman and Daniels wrote about industrial development. There were also some outspoken contemporary critics of the narrow unreality of neo-classical theory, notably Thorsten Veblen, with his sweeping attacks on the exploiting role of 'Big Business', capitalist 'absentee owners', financiers, and advertising salesmen, and his lauding of scientists and technologists as the true creators of the vast (but still artificially restricted) increase in modern industrial production and wealth.¹ Doubtless his writings contain a good deal of historical and theoretical nonsense (together with many discerning insights), and he was almost pathologically distorted in his attitude towards 'business men' and their activities, but he was undoubtedly right in his emphasis on the realities of applied science and technology, so utterly neglected by most economists of his day. Particularly interesting from our present point of view was his discernment not only of the effects of applied science upon industry, but also of the reverse effects of industrial-technological developments on science,² interactions which he traced back to the Industrial Revolution, though developing mainly in the modern period.

Veblen's views were beyond the pale of economic orthodoxy, but even among traditional economists there was some awareness of the importance of technology in economic development. Particularly interesting are the views on inventions and patents expressed by some economists in the later part of the

¹ See, for example, T. B. Veblen, *The Theory of Business Enterprise* (New York, 1904); *The Instinct of Workmanship and the State of the Industrial Arts* (New York, 1914); *The Place of Science in Modern Civilization* (New York, 1919); *The Engineers and the Price System* (New York, 1921); *Absentee Ownership and Business Enterprise in Recent Times* (New York, 1923).

² 'Science has flourished . . . somewhat in the same degree as the industrial interest has dominated the community's life. And science . . . has made headway in the several departments of human life and knowledge in proportion as these several departments have come into closer contact with the industrial process and the economic interest . . . modern science may be said to be a by-product of the industrial process.' *The Theory of the Leisure Class* (New York, 1899), chap. XIV.

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period.¹ These views, however, were often contradictory. Some emphasized the importance of 'autonomous' or 'spontaneous' invention. Taussig, for instance, considered that great inventors are moved mainly by 'the instinct of contrivance' – 'an inborn and irresistible impulse' – although he recognized the powerful influence of pecuniary gain or profit.² Pigou also considered inventions to be, 'for the most part, spontaneous'.³ Sir Josiah Stamp expressed similar views in a Watt anniversary lecture: 'He [the inventor] is still *sui generis*, and emerges from the ranks of engineers, physicists and chemists, not indeed as a "sport", but as a special product, which is touched by no "economic spring". The sense of curiosity and the idea of fame play a greater part than the economic reward.'⁴ J. B. Clark, on the other hand, emphasized the importance of inducements such as the patent system and profit prospects.⁵ Sir Arnold Plant, similarly, while conceding that some 'amateur' inventors are 'prompted by curiosity rather than hopes of gain', and that some inventions are products of chance or accident, considered that most inventions are 'induced', that they are products of 'the circumstances of time and place'.⁶ One very potent influence was the rate of growth of scientific knowledge; another was the patent system, though opinions differed profoundly on its effects;⁷ but Plant and other economists emphasized the predominance of economic influences, such as relative factor prices, the state of trade, and profit prospects.⁸

II

Neo-classical micro-static theory held sway until after the First World War. The shock of war, however, and the severity of

¹ The best discussion is that in A. Plant, 'The Economic Theory Concerning Patents for Invention', *Economica*, N.S. vol. I (1934), pp. 30–51.

² F. W. Taussig, *Inventors and Money-Makers* (New York, 1915), chap. I.

³ A. C. Pigou, *Economics of Welfare*, 2nd ed. (1924), p. 163.

⁴ Sir J. Stamp, *Some Economic Factors in Modern Life* (1929), p. 113.

⁵ J. B. Clark, *Essentials of Economic Theory* (New York, 1907), chap. XXI.

⁶ Plant, *op. cit.*

⁷ See below, p. 49.

⁸ Cf. the similar conclusions of present-day economists such as Schmookler, below, pp. 25–6.

post-war economic crises, accompanied by widespread and massive unemployment, caused economists to look afresh at the problems of economic growth and fluctuations, and since the Second World War the situation of the 'underdeveloped' countries, and the rivalry between capitalism and communism, particularly in the scientific-technological sphere, have also revived interest in the factors responsible for long-term economic development and growth. There has been renewed concern with those factors previously regarded as 'exogenous', including science and technology, entrepreneurial activity, investment, population, and social structure.

Schumpeter was one of the first to emphasize these long-term aspects, particularly the factors producing economic fluctuations, in which entrepreneurial 'innovations' played a dominant role, whether in new technologies, or new forms of economic organization, new products, new markets, or new resources.¹ He emphasized particularly the importance of major technological innovations, which accounted to a large extent for discontinuous economic growth, in great 'leaps and bounds', e.g. with the introduction successively of the steam engine and its application in cotton, coal, and iron, then the development of railways and steel, followed by electrification, motor transport, and chemicals. Schumpeter tended, however, to neglect the scientific-technological aspects, stressing the entrepreneurial side of innovations rather than technical inventions. The entrepreneur need not be the inventor of the process or product that he introduces, nor need he himself provide the capital: he is essentially an innovator, a business leader, often utilizing the inventions or capital or others, grasping and exploiting potentially profitable opportunities.

In many ways, however, Schumpeter's writings were outside the field of dominant economic theory² and more akin to

¹ J. A. Schumpeter, *The Theory of Economic Development*, first published in German in 1911, translated into English in 1934, reprinted in 1961. See also his *Business Cycles* (New York, 1939), 2 vols.

² He considered that 'economic theory in the traditional sense contributes next to nothing' to the analysis of problems of long-term or historical development (*Theory of Economic Development*, p. 59). Neo-classical theory assumed unrealistically that economic growth was 'a continuous and almost automatic process that does not harbour any phenomena or problems of its own' (*History of Economic Analysis*, pp. 892-3).

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economic history, which, in fact, he personally considered to be more fundamentally important and interesting.¹ New ideas did develop, however, within established economics schools during the inter-war period, with increasingly radical revisions of neo-classical theory.² 'Perfect competition' was abandoned; uncertainty and risk, and the problems of business expectations were brought under analysis; Keynesian macro-economic analysis explored the interconnexions between the aggregates of income and consumption, savings and investment, in a revolutionary new *General Theory*. But this new theory was still static, or very short-run, dominated by the idea of maintaining or restoring 'equilibrium': it excluded long-term 'dynamic' factors affecting economic growth such as capital accumulation, science and technology, population, etc. Post-Keynesian theory, however, developed notably by Harrod and Domar,³ created 'dynamic' models, taking account of changes over time in investment and output, income and savings, with the aim of achieving 'dynamic equilibrium' or a steady rate of growth, the rate of investment being a particularly key factor. But scientific-technological and other largely unknown or non-quantifiable variables were still treated in such theories as 'given', as 'non-economic' or 'exogenous'.

In short-run analysis such assumptions may be justifiable and useful, but in the long-term they are untenable.⁴ Changes in science and technology, for example, certainly cannot be disregarded, since they are crucial to investment decisions and continued growth.⁵ What, in fact, are the links between techno-

¹ *History of Economic Analysis*, pp. 12-13.

² For an intriguing investigation into the beginnings of this 'revolution' in economic thought, see G. L. S. Shackle, *The Years of High Theory, Invention and Tradition in Economic Thought 1926-1939* (Cambridge, 1967).

³ See especially R. F. Harrod, *Towards a Dynamic Economics* (1948) and E. D. Domar, *Essays in the Theory of Economic Growth* (New York, 1957). For an elaboration of the Harrod-Domar analysis, see W. J. Baumol, *Economic Dynamics* (New York, 1951), W. Fellner, *Trends and Cycles in Economic Activity* (New York, 1956), and D. Hamberg, *Economic Growth and Instability* (New York, 1956).

⁴ It is true, as Kuznets, for example, has always emphasized, that 'economic growth is essentially a quantitative concept' (Kuznets, *Economic Growth and Structure*, p. 6), but to *explain* the statistics it is necessary to consider many non-quantifiable factors, often regarded as 'non-economic'.

⁵ As Hirschman has pointed out, investment decisions are very closely related to technical progress and inventions, which do not necessarily occur in a

logical advances and investment, how do they change capital-labour and capital-output ratios, how do they affect employment and income, what determines their rate and direction, how can they be measured or controlled? Mere figures of capital accumulation may be quite misleading, since all capital is not homogeneous – quality is important as well as quantity – and such statistics may not, therefore, truly indicate a country's economic-technological progress. Technological improvements may be capital-saving rather than, or as well as, labour-saving; productivity may be increased by more efficient use of existing capital and labour resources (by economies of scale, better training, improved organization), with relatively little new capital formation; capital investment may be 'deepening' or 'widening', it may be in improved machinery or merely (though not usually) in more of the same; it may be in new, fast-growing industries or in old, slow-growing ones; it may be in more or less capital-intensive sectors; it may be in heavy industry or in housing, in scientific research or in education, roads, etc., with varying effects on productive capacity, some largely unquantifiable.

An associated weakness of these macro-economic models is their aggregative and abstract character: couched in terms of national income, consumption, savings, investment, and output, they take no account of the varied realities at the level of particular industries and firms. A tendency has developed, in fact, to refer to these aggregates as if they moved on their own volition, without human agency, under some sort of impersonal, mathematical compulsion: a tendency to talk in 'compound interest' terms, which one finds in some studies of economic growth, e.g. Patel refers to 'the immense power of compound growth at higher rates',¹ and Rostow typifies a country's 'take-off' as a critical phase when 'compound interest gets built into the society's structure' (his term 'self-sustained growth' is similarly misleading).² With all the statistical mani-

steady stream, so that economic growth is unstable. A. O. Hirschman, *The Strategy of Economic Development* (New Haven, Conn., 1958), pp. 34-5.

¹ S. J. Patel, 'Rates of Industrial Growth in the Last Century, 1860-1958', *Economic Development and Cultural Change*, vol. IX, no. 3 (April 1961).

² W. W. Rostow, *The Stages of Economic Growth* (Cambridge, 1960), p. 36. Rostow does, however, stress the need for 'disaggregation' (see below, pp. 42-3).

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pulation of national aggregates, there is little apparent contact with scientific-technical-industrial reality, little awareness of the complex problems facing the business-man or engineer on both the supply and demand sides.

At the same time, micro-economic theory has also traditionally been dominated by narrow, short-run considerations, as in the theory of the firm, rather than with dynamic factors such as technological change.¹ This raises many complex questions. How, for instance, in the midst of uncertainties, do individual firms make their output and investment decisions? To what extent are technological changes responses to demand, or to what extent do they create new markets? How are they affected by, and how far do they bring about, changes in industrial structure? What are the links between scientific research, technological inventions, and investment?

Faced with these problems, economists have reacted in various ways. Some, still trying to retain theoretical rigour and mathematical techniques, have constructed more complex dynamic models on modified Keynesian-Harrod-Domar or neo-classical lines, including 'disaggregated' or multi-sectoral models of the Leontief type.² These have included technological progress along with land, capital, and labour among the factors of production determining economic growth, but in a highly abstract manner, with many unrealistic assumptions. Other economists, therefore, have abandoned these artificial rigidities

¹ Nordhaus has recently pointed out that 'the conventional theory of the firm . . . has neglected the role of new technology', assuming changes in scientific and technical knowledge to be 'exogenous'. W. D. Nordhaus, *Invention, Growth and Welfare: A Theoretical Treatment of Technological Change* (Massachusetts, 1969). He has therefore constructed a theoretical model including inventive activity.

² Early examples of these new 'growth models' were those of R. Solow, 'A Contribution to the Theory of Economic Growth', *Quarterly Journal of Economics* (February 1956), and 'Technical Change and the Aggregate Production Function', *Review of Economics and Statistics* (August 1957), and N. Kaldor, 'A Model of Economic Growth', *Economic Journal* (December 1957), reprinted in his *Essays on Economic Stability and Growth* (1960). A flood of articles and books soon followed: see, for example, F. Hahn and R. C. O. Matthews, 'The Theory of Economic Growth: A Survey', *Economic Journal* (December 1964). More recent and extensive bibliographies are to be found in the works by Lave, Nelson *et al.*, and Mansfield cited below, p. 17 n. 4, and p. 22 n. 2. There is a very thorough discussion of the literature, theories, and statistical techniques in Lave's book, particularly with regard to technological change.

in favour of a looser framework, combining general concepts with a broader more empirical approach, in the classical tradition. These 'development' economists have also shown considerable sympathy towards inter-disciplinary studies, since many psychological, sociological, political, historical, scientific, and technological, as well as economic, factors are involved in the growth process.¹

From our present point of view, the most significant feature of this ferment in economic growth studies has been the gradually dawning realization among economists that science and technology cannot any longer realistically be treated as 'exogenous' – that applied science is, indeed, the major force behind modern economic growth and must somehow be brought into economic theory. This has been associated with a growing demand for 'disaggregation', for studies of applied science and technology in particular industries and firms, so as to produce more detailed empirical information on which to base developing theory: somehow, it is felt, micro- and macro-economics should be married in a comprehensive dynamic analysis.

Both before and after the 'Keynesian revolution', Schumpeter had, as we have seen, laid strong emphasis upon the importance of technological and other innovations. Another great American scholar, Simon Kuznets, has stressed even more strongly the vital role of science and technology in economic growth. His earliest publications emphasized the 'sectoral'

¹ An outstanding example of this wide approach is provided by W. A. Lewis, *The Theory of Economic Growth* (1955). Another is E. E. Hagen, *On the Theory of Social Change. How Economic Growth Begins* (Homewood, Ill., 1962). Hagen, having become convinced of 'the inadequacy of economic theory' to solve the problems of economic growth, supplemented it with psychology, anthropology, sociology, and history. For other wide-ranging studies, in addition to the previously cited works by Schumpeter, Meier and Baldwin, Higgins, Hoselitz, Kuznets, Rostow, and Supple, see also C. E. Ayres, *The Theory of Economic Progress* (Chapel Hill, North Carolina, 1944); B. S. Keirstead, *The Theory of Economic Change* (Toronto, 1948); M. Abramovitz, 'Economics of Growth', in B. Haley (ed.), *A Survey of Contemporary Economics* (Homewood, Ill., 1952); H. F. Williamson and J. A. Buttrick (eds.), *Economic Development* (New York, 1954); N. S. Buchanan and H. S. Ellis, *Approaches to Economic Development* (New York, 1955); L. H. Dupriez (ed.), *Economic Progress* (Louvain, 1955); C. Clark, *The Conditions of Economic Progress*, 3rd ed. (1957); C. P. Kindleberger, *Economic Development*, 3rd ed. (New York, 1965); H. J. Bruton, *Principles of Economic Development* (Englewood Cliffs, N.J., 1965).

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theory of growth¹ – that at any given period of time one or a few industries are leading sectors, experiencing rapid technological innovation and pulling the rest of the economy along, but that eventually they experience ‘retardation’, after the ‘innovation effects’ have worn off and the possibilities of rapid technological progress have been fully exploited, so that if the economy is to continue to grow, new industries must take up the running. This pattern of sectoral growth has been further demonstrated in Great Britain’s industrial development by Hoffmann and has been taken up and publicized by Rostow, firstly with emphasis on the role of the cotton industry in Britain’s ‘take-off’ during the Industrial Revolution and then, more generally, along lines similar to the earlier Germanic ‘stages of growth’ theories.² Kuznets meanwhile has continued to provide quantitative evidence of growth patterns in different countries, which, he points out, have everywhere been characterized by rising productivity, which is

possible only through major innovations, i.e. applications of new bodies of technical knowledge to the processes of economic production. . . . In these days it is hardly necessary to emphasize that science is the base of modern technology, and that modern technology is in turn the base of modern economic growth. Without the emergence and development of modern science and science-based technology, neither economic production nor population could have grown at the high rates indicated for the last century to century and a half in the developed countries. True, growth of tested knowledge, both scientific generalization and empirical information, and of modern technology based on it, were necessary, not sufficient conditions: knowledge itself does not suffice.

¹ S. Kuznets, ‘Retardation of Industrial Growth’, *Journal of Economic and Business History*, vol. 1 (1929), and *Secular Movements on Production and Prices* (Boston, 1930). He pointed out that ‘questions of industrial development in their general aspects have become suspect to all conscientious economists and statisticians. We find hardly any discussion of the problem in the standard treatises which have appeared since the great days of the English Classical School . . . economic theory proper has restricted itself largely to static problems’, almost totally neglecting ‘the really dynamic elements of economic life’. *Secular Movements*, pp. 2, 323.

² W. Hoffmann, *British Industry 1700–1950* (English translation, 1955), and Rostow’s works previously cited. See below, pp. 42–3.

. . . Yet the growth of science and technological knowledge has distinct patterns of its own; difficult as it is to discern them (particularly for a mere economist), some attempt should be made to do so, for they are relevant to the pattern . . . that modern economic growth exhibits.¹

In attempting to trace these patterns, Kuznets distinguishes between (a) a scientific discovery, an addition to knowledge, (b) an invention, the application of existing knowledge to a useful end, (c) an innovation, the first industrial use of an invention, (d) an improvement, a minor application, and finally (e) the spread of an innovation, usually accompanied by improvements. Between these successive phases there is a feed-back effect, as industrial development and usage may lead to further improvements, inventions, or scientific discoveries. But there are also bottle-necks between them, for all scientific discoveries do not necessarily lead to inventions, all inventions are not exploited, and all innovations are not successful. About these interconnexions, Kuznets points out, there is 'still little known',² but at the point where innovations transform new technical knowledge and inventions into productive use, three factors are important: (a) capital investment, (b) entrepreneurial talents, and (c) the market. Economic growth is thus a product of economic and social as well as scientific and technological factors.

More recently, Kuznets has emphasized even more strongly the importance of science and technology, pointing out the inadequacies of capital investment as a measure of economic growth. The most important capital of an industrially advanced nation, he considers, is not its physical capital, but its human capital, its scientific and technical knowledge, resulting from improved education and training. It might even be possible

¹ S. Kuznets, 'The Meaning and Measurement of Economic Growth', *Six Lectures on Economic Growth* (Glencoe, Ill., 1959), pp. 13-41. Kuznets has written voluminously on this subject. See his recent volumes on *Economic Growth and Structure* (1965) and *Modern Economic Growth* (1966), which include references to his numerous earlier publications.

² This is also the view of those who have been involved in modern 'research and development', e.g. D. A. Schon, *Technology and Change* (New York, 1967), emphasizes the difficulty of differentiating between 'invention' and 'innovation', 'research' and 'development', and points out that the whole process is often viewed as being more clear-cut and rational than in fact it is.

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for technological progress to increase output rapidly 'without any additions to the stock of capital goods'. Capital investment, therefore, cannot be regarded as the one strategic factor. The growth of applied scientific knowledge 'must account, in large part, for unusually rapid rates of economic growth in recent centuries', though it has obviously been closely related to broader economic and social forces: it is this, in fact, which has been 'the strategic factor'.¹

These views have been supported in recent years by an increasing number of other economists. Fellner, for instance, has emphasized the importance of a continued flow of 'technological and organizational improvements', particularly inventions, as the main factor behind long-run growth – the force which has staved off the Ricardian prospect of diminishing returns and a stationary economy – although he discusses their development in narrowly economic terms.² Higgins has put technological progress foremost among the causes of economic growth, linked with entrepreneurial enterprise, acquisition of new managerial and technical skills, and capital accumulation.³ Cairncross has similarly stressed the predominance of technological progress and innovation, while pointing out how little we know about the forces governing them.⁴ Blaug has criticized the unrealistic assumptions in growth theory regarding process-innovations, and suggests that the 'neglect of technical change' can no longer be 'justified by its intractability to the traditional tools of economics'.⁵ Bruton, reviewing contemporary ideas on

¹ *Economic Growth and Structure*, pp. 34–8, 60–2, 70–1, 83–92, 127–30, 194–212. In *Modern Economic Growth*, pp. 8–15, Kuznets expresses a similar view: 'The epochal innovation that distinguishes the modern economic epoch is the extended application of science to problems of economic production.' The most outstanding early example of this was Watt's development of the steam engine. Other early inventions may have been mainly empirical, but Kuznets stresses that 'the intellectual and cultural milieu within which the basic steam inventions were made also produced the burgeoning of modern science and brought about its more extended applications.' His views reflect very strongly the recent emphasis by other economists on the importance of scientific-technological progress, rather than on capital investment. See below, pp. 16–29.

² Fellner, *op. cit.*, pp. 118, 126–9, 137–8, etc.

³ Higgins, *op. cit.*, pp. 202–4.

⁴ A. K. Cairncross, *Factors in Economic Development* (1962), part II.

⁵ M. Blaug, 'A Survey of the Theory of Process-Innovations', *Economica*, N.S. vol. XXX (1963).

the subject in 1960, demonstrated the inadequacy, indeed the non-existence, of any integrated general theory of economic growth.¹ Modern dynamic models, he pointed out, have proved largely unrealistic, because of their artificial assumptions, particularly their exclusion of so-called 'exogenous' factors, which are, in fact, inextricably part of the economic system, such as entrepreneurial behaviour, scientific and technological changes, inventions and innovations, changes in population structure and consumer tastes, labour mobility, trade-union policies, and general social and cultural patterns. He emphasized that 'the growth process must be viewed in a larger context than simply the arithmetic of capital-output ratios, savings-income ratios, and population growth rates'. He stressed particularly the lack of any theory of technological change, and considered that such theory could be developed only from 'case studies of the history of inventions and innovations in different industries'.² In such empirical studies, there could be no valid distinction between 'economic' and 'non-economic' factors: the tools and methods of the economist must be altered and combined with those of the sociologist, psychologist, scientist, engineer, and historian to achieve an all-round understanding. This necessity for inter-disciplinary effort has also been emphasized by Meier, in his investigations into the role of science and technology in future economic and social development.³

III

These criticisms of the inadequacies of growth theory have given rise, during the last decade or so, to a swelling flood of research and publications on the links between science, technology, and modern industrial development.⁴ A few of the more

¹ Bruton, in Hoselitz., op. cit. Kuznets has said much the same thing: *Economic Growth and Structure*, pp. 4-5.

² Reference is made particularly to A. A. Bright, Jr., *The Electric Lamp Industry: Technological Change and Development from 1800 to 1947* (New York, 1949), and W. R. Maclaurin, *Invention and Innovation in the Radio Industry* (New York, 1949). See also A. E. Musson, *Enterprise in Soap and Chemicals* (Manchester, 1965).

³ R. L. Meier, *Science and Economic Development* (New York, 1956).

⁴ There is a very full bibliography in R. R. Nelson, M. J. Peck, and E. D. Kalachek, *Technology, Economic Growth, and Public Policy* (1967). Note particularly

outstanding examples will illustrate the strength of this changing academic current. Professor Jewkes and his collaborators have made a wide-ranging but detailed survey into the sources of invention, dealing with a large number of cases in many different industries, in an effort to explain the forces behind modern scientific technology, 'one, if not the main, spring of economic progress'.¹ Unlike most economists, who tend to assume that applied science is only of twentieth-century importance, they have demonstrated that, in fact, from the Industrial Revolution onwards there has been an increasingly close relationship between science and technology in the making of inventions. They have thus shown – though only in broad outline, since their major emphasis is on the twentieth century – that knowledge of the historical background may serve to dispel many false illusions about technological development. It will tend, for instance, to modify the still widely-held belief in the distinction between 'pure' and 'applied' science, or between theoretical science on the one hand and industrial empiricism on the other. As they suggest, and as has since been demonstrated in great detail,² there were close links between industrialists and scientists even in the eighteenth century; long before the development of modern State education, there were many agencies for the development and diffusion of scientific and technical knowledge. Jewkes and his collaborators have emphasized particularly the importance of individuals in the making of inventions; although in modern times the growth of 'big business' and establishment of corporate research and development organizations have tended to alter the situation, the individual's role is still important. They have emphasized, too, that the underlying motives are by no means entirely

the important collection of articles in R. R. Nelson (ed.), *The Rate and Direction of Inventive Activity* (National Bureau of Economic Research, Princeton, 1962). For more recent publications, see E. Mansfield, *The Economics of Technical Change* (New York, 1968) and *Industrial Research and Technological Innovation* (New York, 1968); W. P. Strassman, *Technological Change and Economic Development* (New York, 1968); and Nordhaus, op. cit.

¹ J. Jewkes, D. Sawers, and R. Stillerman, *The Sources of Invention*, 2nd ed. (1969).

² By Musson and Robinson, op. cit., whose ideas were first published in 'Science and Industry in the Late Eighteenth Century', *Economic History Review*, 2nd ser., vol. XIII, no. 2 (December 1960).

economic, but also psychological and social: 'the intuition, will and obstinacy of individuals spurred on by the desire for knowledge, renown or personal gain [remain] the great driving forces in technical progress'.

Salter likewise has emphasized that merely to measure productivity, as econometricians have done, 'is not to understand', and that 'behind productivity lie all the dynamic forces of economic life: technical progress, accumulation, enterprise, and the institutional pattern of society. These are the areas where our understanding remains rudimentary.'¹ His opinion is that, among the causes of increased production and productivity, 'primary emphasis must be placed on technical progress and economies of scale'. Technical progress has been based on 'improving technical knowledge', both scientific and empirical. It is true that generally accepted 'economic' factors, such as relative prices of capital and labour, and investment, have also been important, but it is 'extremely difficult to distinguish' between economic and technical factors. Changes in relative factor prices may stimulate invention and innovation, but, equally, scientific and technical developments may alter relative factor prices and influence investment decisions. Scientific-technical knowledge and economic factors are continuously changing and interacting in this way, but existing economic theory has failed almost completely to take account of technological factors, so that discussion of the problems of productivity has to take place 'in a theoretical vacuum'.

More recently, Nelson, Peck, and Kalachek in the United States have similarly emphasized 'the leading role' of scientific-technological progress in economic growth, and the vital contribution of education and training in producing entrepreneurs, managers, and workers with scientific and technical competence and flexibility.² But, like Kuznets, while they stress the importance of science and technology as a necessary condition of economic growth, they emphasize that it alone is not sufficient and that inventions are stimulated mainly by economic factors, especially changes in demand, resulting from population

¹ W. E. G. Salter, *Productivity and Technical Change*, 2nd ed. (Cambridge, 1966).

² *Technology, Economic Growth, and Public Policy*.

growth, expansion of exports, changes in income, tastes, etc.¹ Here, of course, they are also following Adam Smith, and, like Salter, they see these market forces operating on technology through changes in relative factor prices, leading to labour-saving or capital-saving inventions (as the case may be) and factor-substitution. They see examples of these causes and effects in the English textile inventions during the Industrial Revolution, beginning with the invention of the fly-shuttle:²

The result was a fall in the price of cloth, more cloth output, and more demand for yarn to make cloth, thus raising economic returns to technical advances in the spinning processes. The profit prospects for successful invention were further enhanced by labour shortages and rising wages since the supply of spinners increased slowly. . . . The work which led to the water frame, the spinning jenny and, later on, the spinning mule . . . was directly stimulated by this increase in demand for yarn and in wages of spinners. This spurt of induced invention in spinning eventually overshot, and shortages of weavers began to materialize. These new shortages were met in part by a shift of labour into weaving, and in part by the shift of attention of inventors from spinning to weaving resulting in the development of the power loom.

¹ They admit that 'many of the technological advances . . . have stemmed, at least in part, from the work of a single man or a small group of men with zeal for an idea and only limited concern for profit, social value, or cost', but point out that these have generally required outside financing and thus came 'within the orbit of economic calculation'.

² Op. cit., p. 29. They do not explain how the fly-shuttle came to be invented; it does not fit in with their demand theory, because the existing shortage was in the spinning not the weaving section. They also draw on H. J. Habakkuk's *American and British Technology in the Nineteenth Century* (1962), to illustrate the different effects on invention of differing factor prices in the two countries, e.g. in Britain, where fuel was relatively scarcer and more expensive, fuel-saving inventions were more important, as demonstrated by the inventions of Watt and Bessemer, whereas in the United States, where labour was relatively costly, labour-saving inventions predominated. Habakkuk's theories, however, have given rise to some argument: see, for example, the review article by D. S. Landes in *Business History*, vol. VII, no. 1 (January 1965). For further examination of these factors, see S. B. Saul, *Technological Change: The United States and Britain in the Nineteenth Century* (London, Methuen, 1970).

Yet while they emphasize this 'demand-pull' effect, they also counterbalance it with the statement that 'a strong demand . . . is not sufficient' to produce technological advance. 'Capability [i.e. supply] is important as well as demand.' This depends on scientific-technical knowledge and its diffusion, material resources, etc. But, unlike Jewkes and his collaborators, they follow most economists in distinguishing the 'science-based' inventions of the twentieth century from the allegedly empirical developments of the eighteenth and nineteenth centuries.¹

Scientific and technological developments are not easily manipulated by economists' statistical tools, but in recent years attempts have been made to quantify them. Some of these efforts have been very strained. Maclaurin, for example, has endeavoured 'to break down the process of technological advance into elements that may eventually be more measurable', with particular consideration of the key 'propensities': to develop pure science, to invent, to innovate, to finance innovation, and to accept innovation.² But some of the proposed indicators while certainly reflecting significant developments, cannot bear much weight of interpretation, e.g. numbers of Nobel prize-winners, scientific publications, etc. Others, such as research expenditure and patents, though requiring careful handling, are undoubtedly useful,³ but there still seems little possibility of directly measuring scientific-technological developments with anything like mathematical accuracy.⁴

¹ See also, for example, Mansfield, *Economics of Technical Change*, pp. 11-12 and 43-5.

² W. R. Maclaurin, 'The Sequence from Invention to Innovation and its Relation to Economic Growth', *Quarterly Journal of Economics*, vol. LXVII (1953). See also his article, 'The Economics of Invention; A Survey of the Literature', *Journal of Business*, vol. XXXII (April 1959).

³ See, for example, in addition to the previously cited works by Jewkes, Salter, Nelson, Mansfield, etc., L. Silk, *The Research Revolution* (New York, 1961); N. E. Terleckyj and H. Halper, *Research and Development* (New York, 1963); J. Bright, *Research, Development, and Technological Innovation* (Homewood, Ill., 1964); R. Tybout (ed.), *The Economics of Research and Development* (Columbus, Ohio, 1965). For patents, see below, pp. 24-8, 49-53, and 115-20.

⁴ Abramovitz, *op. cit.*, for example, though confident that scientific and technical advances account for 'a very large share, if not the bulk, of the increase in output', has admitted that 'measurement of the relation between changes in the stock of knowledge and the pace of economic growth has so far proved impossible'. Adelman, *op. cit.*, has more reluctantly recognized that technological

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Many economists, therefore, have continued to place their main emphasis on economic factors. Blaug, for example, in discussing innovations, points out that firms have to consider not only technical possibilities, but also probable costs, output, sales, prices, and profits. Innovations are 'market-induced'.¹ But he recognizes the prevailing ignorance and the need for 'detailed case-studies of innovating activity' in particular firms and industries (as Maclaurin, Brunton, and others have also urged). 'Until then we shall not be able to choose decisively between the concept of a technically determined "life-cycle of capital-output ratios" and the theory of market-induced innovations.' At the aggregate level there is as yet no possibility of any quantitative assessment: 'No one has yet managed to measure the state of technical knowledge, much less the rate of change of technical knowledge.'

The most recent studies in this field have tended to confirm this observation. Despite intensive econometric efforts, it still remains true that 'there is no way to measure the rate of technological change directly',² even in the present-day economy, and resort has therefore to be had to indirect measurements, such as estimates of 'labour productivity' (output per man-hour) or of 'total productivity' (relating changes in output to those in both capital and labour inputs); but these are not really adequate for the purpose (quite apart from the serious theoretical and practical problems of aggregate measurement). Such analyses of the 'production-function' – an abstract economic-theoretical concept rather than a scientific-technical

progress and socio-cultural factors, though of vital importance, cannot be quantified.

¹ Op. cit. See also his article 'Technical Change and Marxian Economics', *Kyklos*, vol. XIII (1960), in which he similarly emphasizes that technical changes may be capital- as well as labour-saving, that they are, in fact, responses to changes in relative factor prices, and are consequent upon 'market pressures'. But he admits that this theory cannot apply to 'product-replacing or demand-creating innovations for which there is no basis of comparison with previous cost-outlays', and that 'little can as yet be said' about these, or about 'variations in the level of inventive efforts'.

² Mansfield, *Economics of Technical Change*, pp. 15, 33-4. In addition to Mansfield's works, see also M. Brown, *On the Theory and Measurement of Technological Change* (Cambridge, 1966), and L. B. Lave, *Technological Change: Its Conception and Measurement* (Englewood Cliffs, New Jersey, 1966).

reality – are concerned with the effects rather than the causes of technological change.

Econometricians have, however, succeeded to some extent in getting round these problems of measuring technological change directly, by subtracting measurable elements and leaving 'residuals'. Abramovitz was one of the first to discern that much the greater part of the increase in net product *per capita* was associated with something other than inputs of physical capital and labour.¹ Solow estimated that of the increased productivity in the United States from 1909 to 1949, only 10–13 per cent was attributable to increase in capital and that most of it resulted from technological progress.² Fabricant, Domar, and others reached similar conclusions,³ and Denison's broad study of recent American and European growth rates⁴ has shown that, in the USA and UK, for example, education and improvements in technological knowledge have together accounted for between 40 and 50 per cent of total growth – far more than capital investment – and they were also, no doubt, partially responsible for other growth factors, such as increased labour and managerial efficiency, economies of scale, and improved allocation of resources.⁵ Other economists, in both the USA and Great Britain, have reached a similar conclusion,

¹ M. Abramovitz, 'Resource and Output Trends in the United States since 1870', *American Economic Review* (May 1956).

² R. M. Solow, 'Technical Change and the Aggregate Production Function', *Review of Economics and Statistics*, vol. 39 (August 1957). See also his articles on 'Investment and Technical Progress', in *Mathematical Methods in the Social Sciences* (Stanford, 1960), ed. by K. Arrow, J. Karlin, and P. Suppes, and 'Technical Progress, Capital Formation, and Economic Growth', *American Economic Review*, vol. 52 (May 1962).

³ S. Fabricant, 'Economic Progress and Economic Change', *34th Annual Report of the National Bureau of Economic Research* (New York, 1954), and *Basic Facts on Productivity Change* (NBER, New York, 1959); E. D. Domar, 'On the Measurement of Technological Change' *Economic Journal* (December 1961).

⁴ E. F. Denison, *The Sources of Economic Growth in the United States* (New York, 1962), and *Why Growth Rates Differ* (Washington, D.C., 1967). For a similar comparative study, see A. Maddison, *Economic Growth in the West* (1964), which also stresses the importance of science, technology, and education, but places more emphasis on the general level of demand and its effects on entrepreneurial expectations, thus determining investment and technological advance.

⁵ In fact, 'technical progress' defined more broadly could account for 70 per cent or more of the US growth rate. E. F. Denison, 'United States Economic Growth', in P. M. Gutmann (ed.), *Economic Growth* (Englewood Cliffs, N.J., 1964).

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that technological progress has been far more important than capital accumulation in causing economic growth.¹ Their views have been supported by researches into the 'economics of education',² which have led to growing emphasis on the importance of 'human capital' and 'investment in human beings', on the 'stock of knowledge' as against the 'capital stock' of buildings, machinery, etc. The statistical estimates are subject to very wide margins of error, and some economists, such as Solow, have had second thoughts on capital investment, on account of its vital relationship to technological progress (much of which is 'capital-embodied'). An immense and controversial literature has accumulated on this subject. Nevertheless, there is now no doubt whatever of the considerable importance of technical progress, including intangible factors such as educational improvement and growth of scientific and technical knowledge, in the process of economic development.

Another, more direct, approach towards quantitative measurement of technical progress and assessment of its causes has recently been made by Schmookler, using patent statistics as an index of inventions.³ His overall aim is extraordinarily ambitious: to discover 'what laws govern the growth of man's mastery over nature'. He points out that existing economic theory has done little or nothing to solve this problem: 'Technological change is the *terra incognita* of modern economics.'⁴ Recent research has demonstrated that the growth of

¹ Kuznets, for example (see above, pp. 15-16: see also his *Modern Economic Growth*, pp. 63-85). Cairncross, *op. cit.*, has also expressed the view that capital accumulation has probably accounted for no more than about a quarter to a third of economic growth, and that technical progress and innovation have been the real dynamic forces.

² By Mincer, Schultz, Becker, and others in the USA, and by Vaizey, Robinson, and Blaug in Great Britain. See also, on a closely related topic, F. Machlup, *The Production and Distribution of Knowledge in the United States* (Princeton, N.J., 1962).

³ J. Schmookler, *Invention and Economic Growth* (Cambridge, Mass., 1966). Among his various articles, see especially 'Economic Sources of Inventive Activity', *Journal of Economic History*, vol. XXII (1962). For a much earlier use of US patent statistics, in the study of cyclical and secular changes in various industries, see R. K. Merton, 'Fluctuations in the Rate of Industrial Invention', *Quarterly Journal of Economics*, vol. XLIX (1934-5).

⁴ A similar view has more recently been expressed by Mansfield (*op. cit.*, pp. ix, 17), who points out that a decade or so ago 'the economics of technological change . . . was almost totally unexplored', and that 'existing theory is still in a relatively primitive state'.

'intellectual capital', the development and diffusion of new technical knowledge, has been much more important than accumulation of physical capital, and has thus tended to play down the importance of 'economic' factors, since conventional economic theory treats technological progress as 'exogenous', i.e. 'determined by non-economic forces'. But, Schmookler points out, inventions are products of supply and demand: on the supply side they result from accumulated technical knowledge, while on the demand side they are produced for utilitarian purposes, to satisfy consumer wants. He therefore seeks to answer the question whether they are 'mainly knowledge-induced or demand-induced'. And after examination of the patent statistics in several American industries over the past century, he comes down heavily on the demand side, mainly because of the close correlation, both short- and long-term, which he observes between numbers of patented inventions and output (or sales) and investment in the industries concerned, with patents usually lagging slightly behind. Thus he concludes that economic growth determines the rate of invention and technical progress, rather than vice versa, and that economic growth is determined by socio-economic forces, such as the state of the economy, population growth and structure, changes in *per capita* income, etc. It would appear that we have come full circle and are back with Adam Smith again. Inventors, Schmookler considers, do not usually make discoveries by chance: they are influenced by market forces, by prices and profit prospects; they invent 'for gain'. Scientific discoveries and technological knowledge are 'necessary, but seldom sufficient, conditions for invention'.

Schmookler's evidence and arguments are certainly very impressive and compelling. There is no doubt that they will have a generally favourable reception among economic historians, who in recent years have tended to become increasingly Smithian in their emphasis upon demand factors among the causes, for example, of the Industrial Revolution.¹ It must be

¹ See below, pp. 40-42. Among growth economists, moreover, Kuznets is now inclined to accept the view that technological inventions are mainly 'responses to demand', though he still emphasizes the complex interplay between scientific-technological and socio-economic factors. *Modern Economic Growth*, pp. 8-15.

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pointed out, however, that there are some weaknesses in both his evidence and arguments, and that his conclusions cannot be regarded as decisive. Patents do not cover all inventions, nor are all patented inventions of equal importance.¹ Schmookler admits that the patent statistics refer predominantly to minor improvements, not major inventions, and he can only surmise that his conclusions may 'probably' apply to 'basic inventions which establish new industries', though in their case he has uncertainly to postulate 'latent demand' as the determining influence. He does not explain how this latent demand becomes operative, and how inventors are made aware of it: clearly, in the case of a new basic invention, or new product, an inventor cannot be influenced by present output or sales of that product, though he may discern future market possibilities. Generally, in fact, except for establishing the aggregate statistical relationship between patents and sales or output, Schmookler is very brief and vague in his examination of the socio-economic forces affecting demand, and how individual inventors are affected by them. He also admits to a theoretical weakness in his argument, in that, in actual business operations, research expenditure leading to patents and new or improved products must necessarily occur *before* such products are marketed, i.e. before they can affect output and sales figures. Moreover, whereas his statistical evidence shows patents coinciding with or lagging somewhat behind sales, other scholars have pointed to the time-lags, often very considerable, between scientific discoveries and technical inventions and their eventual embodiment in industrial innovations.²

On the supply side, moreover, Schmookler is very vague and inadequate in the evidence which he produces on the motives of particular inventors, especially those of major importance in the industries concerned: his conclusions are mostly deduced from the quantitative aggregates. He makes the significant confession that 'for most of the roughly one thousand inventions in our four categories the literature available unfortu-

¹ For some discussion of the serious limitations of patent statistics, see the contributions by Kuznets and Sanders, with Schmookler's comments, in Nelson (ed.), *The Rate and Direction of Inventive Activity*.

² See below, pp. 27-9, 52-3, and 82-3.

nately fails to identify the initiating stimuli', though in a minority of cases the evidence showed the stimulus to be largely economic.¹ Schmookler also admits that he made little investigation into the role of chance or accident.² He deliberately excludes the motives of scientists.³ He admits that scientific and technical knowledge must, of necessity, be anterior to inventions and to their 'intended, socio-economic, functional *future*'. What, then, originally motivated new scientific and technical knowledge? Is this still to be treated as 'exogenous'? 'In our context [says Schmookler] science and engineering appear as given, to be used to explain but not themselves to be explained. In the larger context, however, these too would require explanation.' He suggests that the explanation might be largely in terms of market demand, to which scientific and engineering progress is probably responsive, but this is obviously debatable.⁴ Is it not, in fact, a vain quest to seek which is more important, demand or supply, markets or technology, when *both* are essential to economic growth and both are continuously changing and interacting?

These criticisms of Schmookler's mainly statistical approach receive support from the views recently expressed by various people with wide and direct scientific and industrial experience. Not only is there historical evidence of long time-lags between

¹ Op. cit., p. 66.

² Ibid., p. 197. We may contrast Schmookler's vagueness in these matters with the earlier findings of Rossman, who in an extensive investigation among professional inventors found that the motive most frequently mentioned was the 'love of inventing', closely followed by 'the desire to improve'; accidental discoveries also played an important role. J. Rossman, *The Psychology of the Inventor* (Washington, D.C., 1931). H. Hart, *The Technique of Social Progress* (New York, 1931), reached similar conclusions: that 'the pleasure of the inventive process, the zest for pitting one's powers against a puzzling obstacle, the fun of using one's mental and mechanical abilities, the satisfaction of rendering a service to one's fellowmen', were pre-eminent motives. See also H. S. Hatfield, *The Inventor and his World*, 2nd ed. (1948), and the views of Jewkes and earlier economists, above, pp. 7-8, and 18-19. On the other hand, there is no doubt that in modern times the individual inventor has tended to be displaced by corporate research in large firms, motivated by economic considerations. But as Jewkes, Nelson, Schon, Nordhaus, and others have pointed out, inventions continue to come from the supply as well as the demand side.

³ Op. cit., p. 177.

⁴ For the debate on the early modern 'Scientific Revolution', see below, pp. 37, and 56. For modern works on the psycho-sociological motivations of scientists, see below, p. 38 n. 1.

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inventions and their commercial exploitation, but even in the modern period the lags appear to have been longer than is often supposed.¹ It has been pointed out that an 'invention tends to come before society needs it or is willing to buy it. Therefore you usually have to wait for the invention to become saleable to the public. More frequently than not you have to wait a very long time. Few inventions are made that a society is willing to pay for immediately.' Entrepreneurial drive, capital, and risks are required to get new products on to the market: there is often strong consumer resistance, and opposition from those whose position is threatened by innovations. The market has to be 'invaded': market research, advertising, etc. are required. In fact, a market has often to be *created* for new products.²

Very similar views, backed by much empirical evidence, have recently been put forward by Schon,³ who emphasizes that even under modern 'research and development' programmes, invention, innovation, and marketing are full of uncertainties and risks. The process of invention takes unpredictable 'twists and turns'; in trying to solve one problem, unexpected discoveries are often made in another scientific-technical field, or discoveries in another field impinge unexpectedly on the problem in hand; costs and results are highly uncertain, and accident plays a significant part. The notion that invention is a direct response to clearly discerned 'needs' is 'a myth', except where small improvements are concerned. In the case of a major new invention or product, the 'need' for it does not pre-exist, and a market has to be developed; 'market research' tends to follow rather than precede technical invention and innovation.

¹ From studies of a number of important modern examples, the average time-lag appears to have been around fourteen years in the period since the Second World War, though having declined considerably during this century. Mansfield, *op. cit.*, pp. 100-3.

² A. W. Warner *et al.* (ed.), *The Impact of Science and Technology* (New York, 1965), pp. 137-9, 211. Schumpeter similarly emphasized the risk-taking role of the entrepreneur in introducing new processes and products, and Kuznets pointed out many years ago that 'demand for tea, cotton cloth, radios, electric light and automobiles appeared only after the progress of technique had made all these commodities available'. *Secular Movements*, pp. 8-9.

³ Schon, *op. cit.*, *passim*. Cf. also the evidence collected by Jewkes and others.

Marketing pressures do not, in any case, automatically bring forth technological solutions, and 'most new products and processes fail'.

Clearly, then, both in theoretical analysis and in statistical investigation, economists are a long way from the precise understanding of the process of economic growth, particularly with regard to technological development. But the broadening of scope and inclusion of long-term variables has brought growth theory into closer relationship with economic history.¹ The eclectic approach of Adam Smith and the classical economists, with their inclusion of long-term factors such as population growth, technological change, etc., and their more general theorizing, has always appealed to economic historians, who have necessarily been concerned with dynamic problems of growth rather than with static equilibrium, and who have also tended to attach more importance to empirical facts, however diverse and complicated, than to simplistic and often unrealistic theorems.² The development of neo-classical micro-static theory tended to create a gulf between economists and economic historians, although, as we have seen, the links were never entirely broken. But the post-Keynesian resurgence of interest in economic growth has brought them much closer together, e.g. the recent common interest in the growth of national income and capital accumulation. On the particular aspects of growth which concern us here – the links between technological progress and economic growth – there is now an obvious possibility of mutual stimulus and collaboration, since economic historians have always laid great emphasis on these interconnexions.

¹ As we have previously noted, however, most economists still repeat the traditional view of the almost entirely empirical character of industrial changes before the twentieth century (see above, pp. 3, 18, and 21). Even the most wide-ranging and erudite of them say the same thing, e.g. Lewis (*op. cit.*, p. 169): 'the great inventions of the eighteenth and nineteenth centuries . . . were all invented by practical people who knew no science, or very little'. Jewkes and his collaborators are remarkable exceptions.

² See, for example, Clapham's famous article, 'Of Empty Economic Boxes', *Economic Journal*, vol. XXXII (1922).

IV

The possibility of an interdisciplinary approach is also opened up by the increasing interest of sociologists in the problems of economic growth and scientific-technological development. It is now many years since Talcott Parsons first drew attention to the shortcomings of economic theory in its neglect of sociological and psychological factors.¹ More recently he has reiterated these views, seeking to integrate economics into a 'general theory of social systems'.² He has stressed the importance of 'non-economic' factors such as social structure, institutions, cultural patterns, values, wants, and motivations. Economic development is thus closely related to social changes, e.g. the growth of modern industry is associated with the break-up of relatively static societies based on hereditary status and custom, and emergence of new social groups rising through economic achievement, with an accompanying revolution in social values and motives, social mobility, and changed attitudes to work, all conducive to exploitation of profit possibilities including technological innovations.³ On the last, however, Parsons merely follows Schumpeter in emphasizing the key role of entrepreneurial enterprise, influenced mainly by consumers' wants and profit prospects.⁴ He refers to 'new ideas' or 'new combinations of the factors of production', to 'technological know-how', and to 'scientific resources potentially available for technological application', but his discussion of these factors

¹ See his articles in the *Quarterly Journal of Economics*, vols. XLVI (1931-2), XLVIII (1933-4), and XLIX (1934-5).

² T. Parsons and N. J. Smelser, *Economy and Society. A Study in the Integration of Economic and Social Theory* (London, 1956). (See also N. J. Smelser, *The Sociology of Economic Life*, Englewood Cliffs, N.J., 1963.) For Parsons's general sociological theories, see *The Structure of Social Action*, 2nd ed. (New York, 1949); *Essays in Sociological Theory Pure and Applied* (Glencoe, Ill., 1949); *The Social System* (Glencoe, Ill., 1951); T. Parsons and E. A. Shils (eds.), *Towards a General Theory of Action* (Cambridge, Mass., 1951); *Sociological Theory and Modern Society* (New York, 1967). Parsons's efforts to combine economic and sociological analysis are in line with the earlier works of Marx, Weber, Durkheim, Pareto, and Veblen.

³ There are obviously close similarities here to Marx's views on the evolution of bourgeois capitalist society and to Weber's thesis of the 'Protestant ethic' and rise of capitalism.

⁴ *Economy and Society*, pp. 43-4, 96-7, 203, 207-9, 264-7.

is terribly vague and repetitive, e.g. technological know-how is 'differentiated into modes of application and adopted to realistic production situations';¹ technological innovation results from 'some dissatisfaction with the current economic mode of activity';² it somehow 'evolves' from scientific knowledge, etc., 'but several stages are necessary before concrete technology results . . . various forms of social organization and concrete factors of production are added to make the facilities fully operative in the production process'.³ There is little evidence here of any contact with industrial-technological reality.

Parsons, in fact, follows Weber in regarding technological progress mainly as an outcome of social changes, changing wants and thus changing market demands.⁴ At one point, however, he does observe vaguely that 'changing processes of production' are not determined solely by 'demand conditions', but also partly by 'changes originating in the conditions of production themselves'.⁵ Here again one notices a similarity to the view of Weber, who recognized that 'this economic orientation [profit-making] has by no means stood alone in shaping the development of technology': a part has been played by imaginative 'dreamers', by purely 'artistic' interests, and other 'non-economic factors'.⁶ Nevertheless, both Weber and Parsons place predominant emphasis on economic and social factors, and refer very sketchily to actual scientific-technological developments. Indeed Parsons has recently expressed a view, now becoming outdated among economists, that technology 'is the outcome of non-economic processes and for economic purposes should be treated as given'.⁷

The importance of socio-cultural factors in economic growth has been increasingly emphasized in recent years by other

¹ Ibid., pp. 131-2.

² Ibid., p. 276.

³ Ibid., pp. 132-3.

⁴ Cf. Weber, *The Theory of Social and Economic Organization* (trans. by Henderson and Parsons, 1947), where there is similar emphasis on the market economy; technological development is considered to be 'largely oriented . . . to profit making' (p. 163).

⁵ *Economy and Society*, pp. 42-3.

⁶ Weber, *loc. cit.*

⁷ Parsons, *Structure and Process in Modern Societies* (New York, 1964), pp. 134-5.

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sociologists, who have made comparative studies of early industrializing Western economies and modern 'under-developed' countries. Hoselitz, for example, has stressed the need for a general theory of growth, which should include social, cultural, and political, as well as 'purely economic' factors.¹ He considers that, in the development of western European capitalism, 'cultural and socio-structural variables . . . created the conditions for economic change'.² Of particular interest, from our present point of view, are his observations that many of the early manufacturers, not only in Britain (e.g. early ironmasters), but also in France and Germany, were 'chiefly contrivers of new technical procedures, rather than men of commercial or financial genius', that many came from humble social origins, were often themselves originally manual craftsmen, and owed much of their success to their technical-innovating and managerial abilities.³

Similarly, Hagen, searching outside the boundaries of economic theory for explanations of economic growth, has stressed the importance of the complex sociological and psychological motives of innovating entrepreneurs, the agents of technological progress. An economist himself, he recognizes that economic thought since the late nineteenth century has completely ignored 'technological creativity', the main cause of economic growth.⁴ And Hetzler, casting aside the prevalent theories of both economics and sociology, has recently become almost phrenetic in his worship of technology and the machine, discerning the sources of technological growth 'in the nature of technology itself', with engineers instead of entrepreneurs as the heroes of the whole process.⁵

Most sociologists, however, tend to maintain a social-determinist point of view. Notable among those who have studied technological development more deeply is Gilfillan, who,

¹ B. F. Hoselitz, *Sociological Aspects of Economic Growth* (Glencoe, Ill., 1960).

² But 'economists may query whether this process of social change is autonomous or whether . . . it is related to changes in the more purely economic variables . . . no causal primacy can be assigned to anyone or any one set of variables' (ibid., pp. 42-3).

³ Ibid., pp. 151-3. See also below, pp. 54-6.

⁴ Hagen, op. cit., pp. 49-51. See below, p. 62.

⁵ S. A. Hetzler, *Technological Growth and Social Change* (1969).

however, opposes Marxist materialist determinism – ‘the idea that invention, technological change, determines economic life and hence all history’ – by putting forward the view that social forces are of most fundamental importance.¹ Technical inventions are an ‘inevitable’ response to changing social needs or demands. His conclusions are thus similar to those of some economists, such as Schmookler, who, however, disagrees with him on some points.² They also lend support to the currently popular opinion among economic and social historians as to the importance of demand factors (changes in population, social structure, and tastes) in the causes of the Industrial Revolution. They therefore merit our serious consideration, as an important example of the sociological approach to this subject.

Gilfillan emphasizes the evolutionary character of inventions and attacks the ‘heroic’ or ‘great man’, theory. Inventions depend not only on past knowledge and practice, but also on numerous interrelated developments in many technological fields: ‘the great inventions are enormous and never-ceasing aggregations of countless inventions of detail’. He therefore criticizes the excessive emphasis on original discovery and the

¹ S. C. Gilfillan, ‘Invention as a Factor in Economic History’, *Journal of Economic History*, Supplement V (1945). See also his earlier book on *The Sociology of Invention* (Chicago, 1935), and his later article on ‘The Prediction of Technical Change’, *Review of Economics and Statistics* (November 1952). Gilfillan, however, was by no means the first scholar to put forward such views. Veblen, for example, while admitting the role of individuals, had stressed that technological improvements are products of previous socio-cultural and technological development in the general community, e.g. in *The Instinct of Workmanship* (1914; new ed. reprint, 1922), pp. 103–4. See also W. F. Ogburn and D. Thomas, ‘Are Inventions Inevitable? A Note on Social Evolution’, *Political Science Quarterly*, vol. XXXVII (March 1922), and W. F. Ogburn, *Social Change*, new ed. (New York, 1950); R. C. Epstein, ‘Industrial Inventions: Heroic or Systematic’, *Quarterly Journal of Economics*, vol. XL (February 1926); W. B. Kaempffert, *Invention and Society* (Chicago, 1930); and H. Hart, *The Technique of Social Progress* (New York, 1931). The ideas, in fact, go back much further, to the eighteenth- and nineteenth-century opponents of the patent system, e.g. the view expressed in *The Economist*, 26 July 1851, that ‘nearly all useful inventions depend less on any individual than on the progress of society. A want is felt . . . ingenuity is directed to supply it; and the consequence is, that a great number of suggestions or inventions of a similar kind come to light.’ See F. Machlup and E. F. Penrose, ‘The Patent Controversy in the Nineteenth Century’, *Journal of Economic History*, vol. X (May 1950). See also below, pp. 37, 45–9, 52–6, and 61–2.

² Schmookler, op. cit., chap. X.

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neglect of subsequent improvements. 'Big starting ideas come cheap and are often duplicated; it is the vast labour of development that is most worth while.'

There is undoubtedly much substance in these views. In marine engineering and shipbuilding, for example, as Gilfillan demonstrates, there are many claimants to the original invention of the steamship, including Fitch, Rumsey, and Fulton in America, and Symington and others in England and elsewhere, and these inventions depended on prior or parallel inventions in the manufacture of iron, engineering, and steam-engine construction. Similarly in the evolution of the steam engine, many scientists and engineers were involved, including various seventeenth-century figures as well as Newcomen and Watt in the eighteenth, and they also depended upon related improvements in construction of boilers, cylinders, etc. Nor is there any doubt that the later, very substantial improvements of Newcomen's engine by Beighton, Smeaton, and others, and of Watt's engine by Hornblower, Trevithick, Wolf, etc., including development of the high-pressure engine, have been comparatively neglected. Similarly, how little we know about the later improvements in spinning and weaving machines, after the initial inventions of Kay, Paul and Wyatt, Hargreaves, Arkwright, Crompton, and Cartwright. It is also right to ask how original were some of the 'heroic' inventors, and how much they were helped by comparatively obscure men. Gilfillan points out that undue importance is attached to those who managed to secure patents or who made inventions commercially successful. That there is much truth in these observations is certainly evident to anyone who has waded through the voluminous reports of patent cases, such as those of Arkwright, Watt, and Tennant, with all the legal and technical wrangling about what constitutes a 'new manufacture' or original invention and all the conflicting evidence by rival claimants.¹

Gilfillan's general theory, however, is open to question, and some of his supporting arguments are themselves contradic-

¹ For a recent study of the legal cases concerning Watt's patent, see E. Robinson and A. E. Musson, *James Watt and the Steam Revolution* (1969). For the weakness of Tennant's claims, see the chapter on chlorine bleaching in Musson and Robinson, *op. cit.* For Arkwright, see below, p. 47.

tory. For instance, how is it possible, by his own social-evolutionary theory, for there ever to be any 'big starting ideas'? And, if such ideas are admitted, do they, in fact, 'come cheap' and easy, as he says? Gilfillan considers that the falsity of the 'great-author theory of invention' is demonstrated particularly by 'the frequency of duplicate inventing, when two or more seekers make the same invention about the same time independently'.¹ There are, of course, many such examples, but is it right to conclude that no inventor is indispensable, that 'if the great So-and-so had died in infancy we should have got his inventions just the same', through someone else? Are inventions 'inevitable', do social forces determine that somehow or other, somebody or other, at some time or other, will produce solutions to technical problems (if not the same inventions then others 'functionally equivalent')? Is it possible, as Gilfillan claims, to state with any certainty what would have happened in the past if, say, the steam engine, or the powerloom, had not been invented? And is it likewise possible, as he also claims, to predict the technological future with any accuracy?² Gilfillan's own technological forecasts have not been very impressive, nor can one place much confidence in his musings as to what might have happened in the past. (It is interesting, however, to compare Gilfillan's notions with the idea of 'counter-factual propositions' more recently put forward by Fogel and others, in regard, for example, to what might have happened if the railways had not been built in America.)

Gilfillan has to admit that the time factor is important, that history might have been different if an invention had not been made when it was. He also casts doubt on his own what-might-

¹ This parallelism in scientific discovery and technical invention had been earlier demonstrated by Ogburn and Thomas, *op. cit.*, and had been noticed by many people since the late eighteenth century. See also R. K. Merton, 'Singletons and Multiples in Scientific Discovery', *Proceedings of the American Philosophical Society*, vol. 105 (1961).

² It has been pointed out that this theory of social 'need' and inevitability 'gives better hindsight than foresight', and that it ignores the fundamental role of the individual, influenced by a complex of social and psychological motivations. H. J. Barnett, *Innovation: The Basis of Cultural Change* (New York, 1953), chap. IV. It also neglects the question of time-lags (see above, pp. 26-9, and below, pp. 52-3).

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have-been surmises: 'imagined unrealities mean trouble aplenty when we attempt to mix them with the complex vastness of real economic history'. Moreover, such surmises clash with his social-inevitability theory: the invention of steam power, for example, '*was* made; and it *had* to be made', at that time, because of 'the advances of population, wealth, technical knowledge, and related sciences'. Gilfillan admits, however, 'the value and contribution of the inventor': that 'inventors as a class are completely indispensable'. Inventive men are in short supply: 'Particularly scarce are men with the high quality of the great inventors, imagination, insight, learning, tenacity, persistence under great obstacles, sometimes no recompense for years. Such men deserve high rewards. . . . They merit credit as well as cash; but not credit for doing what they never did.' This statement is strangely contradictory to his main argument. If inventions are the inevitable products of economic and social forces, if there are usually several claimants to any invention, if an invention (or its 'functional equivalent') would have been made by somebody or other, and if 'big starting ideas' come easily, how is it possible to admit the existence of 'great inventors'? On the other hand, if their existence and value are admitted, how can one argue against giving them 'credit for what they never did', if in fact they *did* do something original and important? In regard to the patent system, Gilfillan is equally contradictory. In places he condemns it, because of its support of 'the great-author theory', because of the duplication of inventions, dubious claims, etc., but elsewhere he refers to it as 'the chief institution for paying for the invaluable work of invention', and largely responsible for the increasing number of inventions in England in the eighteenth century. If invention is the inevitable product of social forces, and individual inventors are of no account, why should it be necessary to encourage them in this way?

Gilfillan eventually concedes that 'the only concept of history that will hold water is one of a network of causation, with social, technic, biologic, geographic, and accidental factors intermingled and each causing the other, so that we cannot properly speak of the primacy of anything'. He admits, for example, that in the early modern industrial development, the

Renaissance and the growth of science were significant, and that 'Watt and most others of the early great had considerable scientific training'; but he does not regard the role of science as really important until the twentieth century, when individual inventors became submerged in corporate research and development.

Other sociologists, while adopting similar social-determinist views of historical development, have placed much greater emphasis on the role of science. Merton, for example, has similarly attacked the 'great man' or 'heroic' theory, stressing the determining importance of social forces in scientific discovery and applied science, as evidenced, for example, in the 'Scientific Revolution' of the sixteenth and seventeenth centuries, springing from the Renaissance and Reformation and motivated by social and religious changes, with strong emphasis on utilitarian objectives.¹ His ideas have been strongly challenged by some historians of science,² who maintain the distinction between 'pure' and 'applied' science, and who consider that the development of scientific ideas has been little influenced by either social or industrial changes. The controversy between Merton, Hall, and others in regard to the 'Scientific Revolution' has developed from the earlier debate on the 'Protestant ethic' and the rise of capitalist individualism, as expressed in the works of Weber, Tawney, and Robertson. Marxist historians, of course, such as Bernal, stress the close interconnexions between scientific, industrial, and social developments, in both the early modern and modern periods,³ while others have tried to combine both points of view.⁴

¹ R. K. Merton, 'Science, Technology and Society in Seventeenth-Century England', *Osiris*, vol. IV (1938). See also his *Social Theory and Social Structure*, rev. ed. (Glencoe, Ill., 1957), part IV, 'Studies in the Sociology of Science'; his article on 'The Role of Genius in Scientific Advance', *New Scientist*, 2 November 1961; and the one referred to above, p. 35 n. 1.

² Most notably by A. R. Hall. See, for example, 'Merton Revisited, or Science and Society in the Seventeenth Century', *History of Science*, vol. II (1963).

³ J. D. Bernal, *The Social Function of Science* (1939; reprinted 1967), and *Science in History*, 3rd ed. (1965). Veblen had expressed similar ideas much earlier (see above, p. 7).

⁴ R. Taton, for example, while stressing the role of individual 'genius' and 'originality' (*Reason and Chance in Scientific Discovery*, trans. by A. J. Pomerans, 1957, *passim*), at the same time admits that 'a considerable number of discoveries were made almost simultaneously by different scientists, working independently',

V

Our review of economic and sociological theories concerning the role of science and technology has demonstrated the increasing awareness of the importance of these factors in economic growth. At the same time, it has revealed that, while certain general theoretical propositions have been produced, together with many shrewd insights based on empirical research, there certainly does not exist anything like an agreed general theory, integrating science and technology into the older theories of economic growth. The view of many economists and sociologists appears to be, in fact, that in the present state of ignorance progress can only be achieved through more detailed case-studies of particular industries, firms, and inventions, i.e. by 'disaggregation'. It also seems evident that, despite efforts to achieve statistical precision, through analysis of patent statistics, research and development expenditure, capital investment, etc., the sources of scientific discovery, technological invention, and industrial innovations are unlikely, in the near future (if ever), to be reduced to quantifiable terms, especially at the aggregate level; indeed, it would appear not unlikely that sociological and psychological analysis may be able to provide as much insight as economic theory and statistics, since the underlying motives are by no means entirely economic.¹

and that such discoveries 'often arise when the general level attained by the science of the times renders them almost inevitable' (ibid., p. 108); moreover, scientific discovery may be regarded as 'the reflection of the civilization of an epoch', with political and economic factors playing an important role (ibid., p. 155). See also E. G. Boring, *History, Psychology, and Science* (ed. R. I. Watson and D. T. Campbell, New York, 1963), part 1, where the importance of 'the psychosocial matrix' is emphasized, but the existence of individual genius is affirmed. For Usher's views, see below, p. 49.

¹ In addition to previously cited works on sociological and psychological motivations of scientists, inventors, and entrepreneurs, see also the following: P. A. Sorokin, *Social and Cultural Dynamics* (New York, 1937); A. Kardiner, *The Psychological Frontiers of Society* (New York, 1945); B. Malinowski, *The Dynamics of Culture Change* (New Haven, Conn., 1945); J. B. Conant, *On Understanding Science* (New Haven, Conn., 1947); D. H. Killeffer, *The Genius of Industrial Research* (New York, 1948); B. Barber, *Science and the Social Order* (New York, 1952); B. F. Skinner, *Science and Human Behavior* (New York, 1953); H. H. Anderson (ed.), *Creativity and Its Cultivation* (New York, 1959); K. R. Popper, *The Logic of Scientific Discovery* (Toronto, 1959); D. C. McClelland, *The Achieving*

Nevertheless, examination of developing theories does provide us with a framework of ideas and many intriguing questions for application to economic history. What contributions did science and technology make to the Industrial Revolution? To what extent were technological changes scientific or empirical? What agencies were there for general, technical, and scientific education and for the diffusion of technology? To what extent was the Industrial Revolution knowledge-induced, or the result of 'an instinct of contrivance', or the product of market forces? What were the links between changing technology and changes in population, social structure, incomes, and tastes, or changes in export markets? What parts were played by capital, labour, and natural resources, and what changes occurred in the price-relationships between these factors of production? Were technological changes labour-saving or capital-saving or both? What statistics are there of capital investment, patents, etc.? What was the role of the patent system? What distinction can be discerned between inventions and innovations? What were the motives of inventors and entrepreneurs? Was there a general advance, all along the line, or can we distinguish 'leading sectors', spearheads of technological advance? Did the changes occur with dramatic suddenness, were they revolutionary, or were 'preconditions' gradually established and was the process evolutionary? What evidence is there for the sociological theory of 'inevitability', or for economic or scientific-technological determinism?

To answer all these wide-ranging questions adequately – if they *can* be answered – would require volumes, rather than these few pages. Indeed, one could fill a sizeable library with relevant books that have already been written. We shall not endeavour to deal here with the commercial aspects, such as expanding trade and marketing, or with social aspects such as the growth of population and changing social structure, which have been the subjects of other volumes in this series.¹ Our

Society (Princeton, N.J., 1961) and *Motivating Economic Achievement* (New York, 1969); B. T. Eiduson, *Scientists: Their Psychological World* (New York, 1962); C. W. Taylor and F. Barron, *Scientific Creativity: Its Recognition and Development* (New York, 1963).

¹ D. M. Hartwell, *The Causes of the Industrial Revolution in England* (1967);

particular concern will be with the interactions between science, technology, and economic growth during the Industrial Revolution, though stressing all the time that these cannot realistically be considered in isolation.

The prevalent tendency among economic and social historians is to place more emphasis than hitherto upon the demand side, upon market forces at home and abroad. It is a good many years now since Ashton pointed out that the popular view of the Industrial Revolution as 'a wave of gadgets' is no longer tenable,¹ and more recent work has tended to reinforce his opinion. Habakkuk, in particular, has strongly affirmed that although 'the main stimulus to growth came from changes in industry', Adam Smith's explanation of these changes is 'still the most reasonable': that they resulted from the increase of trade, extension of the market, increases and shifts in demand, which led to more division of labour, greater efficiency, invention and improvement of machines. 'Most of the economically important inventions of the Industrial Revolution period can more plausibly be ascribed to the pressure of increasing demand rather than to the random operation of the human instinct of contrivance, change in factor prices, or the Schumpeterian innovator (who became an important agent of advance only at a relatively late stage).' True the effects of trade expansion were 'enhanced by changes in religious belief' which stimulated business achievement, and also 'by accessions of scientific knowledge', but 'extension of the market' was the real driving force: 'there is still a good deal to be said for the old view that the acceleration of economic changes in the late eighteenth century was primarily the result of the great expansion of overseas trade in the two preceding centuries'.²

Supple similarly has placed the main emphasis on economic and social factors – population, social structure, markets, entrepreneurial ability, etc. – and considers that 'the older

W. E. Minchinton, *The Growth of English Overseas Trade in the Seventeenth and Eighteenth Centuries* (1969); M. Drake, *Population in Industrialization* (1969).

¹ T. S. Ashton, *The Industrial Revolution 1760–1830* (1948), p. 58 *et passim*.

² H. J. Habakkuk, 'The Historical Experience on the Basic Conditions of Economic Progress', in L. H. Dupriez (ed.), *Economic Progress* (1955).

interpretation of the British Industrial Revolution, which sees it as the outcome of an almost spontaneous wave of heroic inventions, is no longer fully tenable'. But he clearly recognizes the importance of technology: 'The Industrial Revolution may not have been "caused" by, but it was certainly carried forward by, the application of machinery and power to textile production, by advances in the technology of iron manufacture, by the invention of the steam engine and the birth of engineering and the machine-tool industry, and by the overhauling of methods in a host of other industries.'¹

The most recent general survey has produced much the same conclusions:² 'in every instance it was the general movement of the economy, and not the technical innovations as such, that dictated the pace of change. The inventions came – or at any rate were seriously developed – when economic conditions were ripe', i.e. when market demand was growing both at home and abroad, and when the resultant industrial expansion was putting pressure on traditional methods and resources (e.g. of wood fuel and water power). But, of course, demand might have gone unsatisfied, had it not been for the dynamic force of capitalist enterprise, stimulated in Britain by changing class structure, social mobility, the profit motive, and free competition. However, in Lilley's view, once the Industrial Revolution got under way, it gradually acquired an almost independent technological impetus, in which applied science played an increasingly important role.

Lilley's conclusions are based on a number of general studies of the Industrial Revolution which have appeared in recent years.³ Most of these, with varying emphasis, stress the funda-

¹ Supple, *op. cit.*, p. 35.

² S. Lilley, 'Technological Progress and the Industrial Revolution 1700–1914', in the Fontana *Economic History of Europe*, vol. III (1970), chap. 3.

³ For example, D. S. Landes, 'Technological Change and Development in Western Europe, 1750–1914', in the *Cambridge Economic History of Europe*, vol. VI (1965), since revised and extended in *Prometheus Unbound* (1969); M. W. Flinn, *The Origins of the Industrial Revolution* (1966); R. M. Hartwell (ed.), *The Causes of the Industrial Revolution in England* (1967); Phyllis Deane, *The First Industrial Revolution* (1967); and various technological histories, such as A. P. Usher, *A History of Mechanical Inventions* (1929; rev. ed. 1954); C. Singer *et al.* (ed.), *A History of Technology* (6 vols., 1954–8), condensed by T. K. Derry and T. I. Williams, *A Short History of Technology* (Oxford, 1960); W. H. Chaloner and A. E. Musson, *Industry and Technology* (1963); M. Kranzberg and C. W. Pursell,

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mental importance of expanding markets, and there would now seem to be little doubt that growing demand was the main driving force behind technological change. But, as in most intellectual swings of opinion, there is danger of reaction going too far. There is evidence that, even two centuries ago, as in modern times, leading innovators, such as Boulton or Wedgwood or Manchester cotton manufacturers, to a very considerable extent *created* the markets for their new products by vigorous sales promotion.¹ No doubt in making technological advances they had strong commercial motives, but invention and innovation often preceded exploitation of market possibilities. And without the technological advances, which made possible greatly increased production at lower costs, markets could not possibly have expanded as they did. There are two blades to the scissors of supply and demand.

Among recent historians of economic growth, Rostow has acquired most notoriety, with his terminology of 'take-off', 'leading sectors', 'self-sustained growth', and other 'stages' of development. Many earlier economists and historians, however, had developed similar 'stages' theories, and many had previously emphasized the role of cotton in the Industrial Revolution. Rostow adds little or nothing in detailed factual information. He refers, for example, to the key roles of science and technology in the 'take-off', but says remarkably little about them; his main emphasis is upon economic and social factors, especially the postulated growth in the rate of investment as a proportion of national income (or net national product) from around 5 to over 10 per cent.² He points out, however, that the

Jr. (ed.), *Technology and Western Civilization*, vol. 1 (1967). See also Lilley's earlier *Men, Machines and History*, 2nd ed. (1965).

¹ N. McKendrick, 'Josiah Wedgwood: An Eighteenth-Century Entrepreneur in Salesmanship and Marketing Techniques', *Economic History Review*, 2nd ser., vol. XII, no. 3 (April 1960); E. Robinson, 'Eighteenth-Century Commerce and Fashion: Matthew Boulton's Marketing Techniques', *ibid.*, vol. XVI, no. 1 (August 1963); M. M. Edwards, *The Growth of the British Cotton Trade, 1780-1815* (1967).

² The references below are from *The Stages of Economic Growth* (1960). But a similar impression is gained from *The Process of Economic Growth* (1953; rev. ed. 1960), e.g. his discussion in chaps. II and III of the 'propensities' to develop and apply science is extremely vague, while in chap. XII, dealing with the 'take-off', he deliberately sets aside 'the question of how new production techniques are generated from pure science and invention', a really colossal omission.

use of such aggregate economic statistics 'tells us relatively little of what actually happens and of the causal processes at work . . . nor is the investment-rate criterion conclusive'. He therefore stresses the need for 'disaggregation', or sectoral analysis: to see 'how rapidly growing manufacturing sectors emerged and imparted their primary and secondary growth impulses to the economy'. On these developments, however, Rostow is very brief and vague. He emphasizes particularly 'the supply of loanable funds' and 'the sources of entrepreneurship', social mobility, and the profit motive, though he leaves aside 'the question of ultimate human motivation' and hints at the need for 'further empirical research' on the motives of entrepreneurs. He talks about different 'growth sectors', about 'the introduction and diffusion of changes in the cost-supply environment', and about 'new production functions', but he provides no details whatever about the scientific and technological developments upon which industrial expansion was based, or about particular individuals or firms which played leading roles. One gets the overwhelming impression of vague, repetitive jargon and generalization, though Rostow's work is certainly of value for its demonstration of the necessity for an interdisciplinary approach to the problems of economic growth – involving consideration of psychological, social, political, and historical, as well as economic factors – and of the necessity for more empirical research.

Rostow's stages theory includes the notion of 'pre-conditions' for 'take-off', but, here again, much work had already been done, and more has since been done, on developments before the Industrial Revolution. Markets had been expanding, with slowly rising population and exports, in the sixteenth and seventeenth centuries. Nor was technological change new or revolutionary. Indeed, there appears to be little justification, either in terms of capital formation or technological development, for Rostow's sharp differentiation between the 'pre-conditions' and 'take-off' periods.¹ Studies in the history of technology show that even in the Middle Ages there had been

¹ See Kuznets's criticisms in 'Notes on the Take-off' in Rostow (ed.), *The Economics of Take-off into Sustained Growth* (1963), reprinted in Kuznets, *Economic Growth and Structure*. See also below, p. 53.

important developments in the application of water and wind power (e.g. in grinding corn, working forge-hammers, etc., and fulling cloth: Professor Carus-Wilson has even discerned 'an industrial revolution of the thirteenth century'¹); there had been improvements in textiles (e.g. the introduction of the spinning wheel and development of the handloom); while in the iron industry the blast-furnace and cast-iron had been introduced in the later Middle Ages. Technological change continued in the sixteenth and seventeenth centuries: Professor Nef's 'industrial revolution' in that period may not have been as revolutionary as he at first suggested,² but he was obviously right to emphasize the importance of the growing British coal industry and of new coal- or coke-burning processes in the manufacture of glass, bricks, salt, sugar, etc.³ Meanwhile, the 'new draperies' were being introduced, together with further improvements in spinning and weaving; these, along with the invention of the stocking frame, ribbon loom, and gig-mill (for raising the nap), foreshadowed the textiles revolution of the eighteenth century. In the metallurgical industries, moreover, the use of coal or coke fuel in the smelting of copper and lead similarly preceded the better-known discoveries of Darby, Cort, and others in iron smelting and forging. Likewise, one can trace earlier important if not revolutionary changes in printing and paper-making, in clock- and instrument-making (with development of precision metal-working tools, such as drills, lathes, and 'wheel-cutting' or gear-cutting 'engines', preparing the way for the later development of heavy mechanical engineering), and also in embryo chemical and allied manufactures producing alum, dyestuffs, saltpetre, acids, potash and soda, soap, etc. From a technological point of view it is very difficult to discern anything really revolutionary in the eighteenth century: even the steam engine was a seventeenth-

¹ E. M. Carus-Wilson, 'An Industrial Revolution of the Thirteenth Century', *Economic History Review*, vol. XI (1941).

² J. U. Nef, *The Rise of the British Coal Industry* (2 vols., 1932), and 'The Progress of Technology and the Growth of Large-scale Industry in Great Britain, 1540-1640', *Economic History Review*, vol. V (1934).

³ His views have recently received support from W. Rees, *Industry before the Industrial Revolution* (2 vols., 1968), mainly concerned with Welsh coal and metallurgical developments.

century product of scientific theory and experiment. 'The early stages of the Industrial Revolution – roughly up to 1800 – were based very largely on using medieval techniques and on extending them to their limits.'¹ Water power, for example, long continued to be much more important than steam power.²

It would appear, then, that technological change was a long-continued response to gradual expansion of markets. But Coleman's researches into the 'new draperies' suggest that the origins and development of these early technical changes are very difficult to trace, and that one should not jump too quickly at the apparently more obvious commercial explanations.³ The same is true of the Industrial Revolution. It is very difficult, because of lack of evidence, to discover what precisely were the motives of individual inventors: in most cases we simply do not know exactly what impelled them to invent new machines or processes, whether it was expanding trade and profit prospects, or a desire to rise in society, or 'an instinct of contrivance', or an interest in mechanical or chemical experiment, or an innate desire to 'improve', or some other psychological impulse. Very few inventors of that period have left letters, diaries, or other evidence explaining their motives.

The sociological theory of the 'inevitability' of inventions, like the modern emphasis on demand among economic historians, leaves many questions unanswered when applied to the Industrial Revolution. It tends to assume an almost automatic response to market or social forces, and greatly to simplify, or even ignore, precisely how and with what difficulties 'supply' was revolutionized. There is an assumption that if economic and social factors are 'ripe', then hey presto! technological solutions will somehow be produced by some-

¹ Lilley, *Technical Progress*, p. 8.

² Ibid., pp. 8–9, and Musson and Robinson, *op. cit.*, pp. 67–71.

³ Coleman has shown how difficult it is to disentangle the complexities of supply and demand, of the new techniques and their inventors, of changing markets, prices and profits, and of technical diffusion, migration, etc., in this field: 'economic causes alone may well be inadequate to explain' these changes, and 'anyone going model-building in the history of new products needs to walk with more than usual care.' D. C. Coleman, 'An Innovation and its Diffusion: the "New Draperies"', *Economic History Review*, 2nd ser., vol. XXII, no. 3, (December 1969).

body or other. Such a view is naïve and unhelpful. It is akin to easy modern assumptions that somehow, someday, Man will reach the planets or the stars, or cure cancer, or perform other scientific-technological miracles. But such assumptions are of little help to the scientists or engineers who are faced with the detailed problems of finding out precisely how to achieve such goals. In the same way, there is danger of easy historical hindsight: we *know* that certain inventions were made during the Industrial Revolution, and it is easy to produce arguments as to their 'inevitability', though they certainly did not seem 'inevitable' to the contemporaries concerned. James Watt (or somebody else), it is said, was bound to invent the separate condenser, etc., because the economic situation 'demanded' it. But exactly how was this demand transmitted to Watt? What evidence is there that he personally was impelled by market forces? What about the qualities of his own individual mind and personality? Was he a 'genius' or not? What were his scientific and technical interests? How did he come to tackle the problem of steam power, and how did he overcome the technical difficulties involved? About Watt, in fact, we do know a good deal,¹ and it is clear that, while he was certainly indebted to existing technology and scientific knowledge, he also carried out a series of original experiments, mainly out of scientific-technical curiosity, with no apparent economic motive in the first place, and that he combined careful experiment with extraordinary insight and imagination. There is equally no doubt that he very soon became aware of the economic possibilities of exploiting his invention. The same is also true of his later development of chlorine bleaching, regarding which we also have his surviving letters;² but his prolonged experiments and disappointments demonstrate the naïve over-simplicity of an after-the-fact theory of 'inevitability' in this case.

The evidence regarding Watt and other inventors also shows the difficulty of distinguishing between invention and innova-

¹ See Musson and Robinson, *Science and Technology . . .*, and *James Watt and the Steam Revolution*; also E. Robinson and D. McKie, *Partners in Science* (1969). See also below, pp. 62 and, 108-9, for Watt's scientific interests.

² Musson and Robinson, *Science and Technology*, chap. VIII.

tion (or commercial exploitation).¹ Watt not only invented the separate condenser, but was also involved in the prolonged technical, financial, and managerial problems of developing it, and development required further inventions or improvements. Watt, in fact, regarded the development problems as the more difficult, not only in his own case, but also in that of Arkwright, in supporting whose claims he declared that, 'whoever invented the spinning machine, Arkwright certainly had the merit of performing the most difficult part, which was the making it useful'.² The water-frame appears to have been invented originally by Thomas Highs (if it was not an improvement on the earlier roller-spinning machine of Paul and Wyatt), but it was not successfully developed, either technically or commercially, until Arkwright himself took it over (almost certainly from Highs); but Arkwright appears to have shown considerable technical ingenuity if not originality (helped though he undoubtedly was by Kay and others), as well as commercial acumen, and further technical inventions and improvements were necessary to the water-frame's eventual success.³ Arkwright, however, unlike Watt, seems to have been driven from the beginning almost entirely by commercial motives, being well aware of the profit potential of a successful spinning machine.

Research into other inventions similarly reveals the complexities of the processes and motivations involved. But too often historians, economists, and sociologists have produced simple explanations, based mostly on inference or deduction from some general theory of economic or social causation, with inadequate empirical evidence. Other scholars, however, have stressed the rare intellectual qualities of outstanding inventors and innovators, such as Watt or Arkwright. Lewis, for example, considers that such men are probably products of

¹ As A. P. Usher has pointed out, 'Technical Change and Capital Formation', in M. Abramovitz (ed.), *Capital Formation and Economic Growth* (Princeton, 1956). See also Schon's views regarding modern 'research and development', above, pp. 28-9.

² A. P. Wadsworth and J. de L. Mann, *The Cotton Trade and Industrial Lancashire 1600-1780* (Manchester, 1931), p. 492.

³ Unpublished paper by A. E. Musson, read to the Midlands section of the Institution of Mechanical Engineers, Birmingham, 1969, on the occasion of the bicentenary of the Watt and Arkwright patents.

‘biological accident’, or ‘statistical accident’, though he recognizes the influence of circumstances, and Hagen also emphasizes the importance of superior innate intelligence, imagination, and energy, though these are affected by socio-cultural factors.¹ Recent research into a great many of the engineers, chemists, etc. who played leading roles in the Industrial Revolution reveals the complexity of factors involved:²

Although economic and social factors were undoubtedly of immense importance in motivating scientific and technological changes – growth of population, expansion of trade, development of transport, availability of capital and credit, social mobility, and the profit motive all impelled or encouraged men to develop new industrial techniques – we have been impressed by the fact that many of the leading scientists and scientifically-minded industrialists were motivated also to a considerable extent by innate curiosity, by a desire to discover more about how industrial processes worked, by an urge to make improvements, and to be esteemed by their fellows, not merely for the money they made, but [also] for their contributions to scientific and technological advance. This impression tends to suggest, indeed, that psychology and sociology may have as much as economics to teach us in such matters.

These conclusions are very similar to those of Professor Jewkes and his collaborators, resulting from equally detailed investigation into the sources of modern invention.³ There is, in fact, plentiful evidence to support Schumpeter’s view that entrepreneurs were motivated not simply by a hedonistic desire for profits, but also by a will to achieve, to acquire power and renown, to found a ‘dynasty’, and other psychosociological drives.⁴

Few, if any, scholars nowadays would subscribe to a naïve

¹ Lewis, *op. cit.*, p. 55; Hagen, *op. cit.*, *passim*.

² Musson and Robinson, *op. cit.*, p. 8.

³ See above, pp. 18–19.

⁴ Cf. also the views of Hagen and McClelland, who (in their works previously cited) similarly emphasize the ‘need to achieve’ and ‘urge to improve’, though they also recognize that economic and social circumstances and opportunities are important.

'heroic theory' of inventions. Most would agree with Usher¹ that technological progress is not autonomous but is a social process, influenced by the economic and cultural environment, and that an invention is not an isolated achievement, but the culmination of a long process of improvement, often achieved only after repeated trial-and-error, and often also requiring many modifications before coming into practical use. This is certainly true of the famous textile and metallurgical inventions of the Industrial Revolution, the development of the steam engine, sulphuric acid and soda manufacture, etc. But if one studies at first-hand the detailed contemporary evidence – revealing the prolonged thought, experiments, disappointments, and innumerable practical problems involved in producing an invention, from the first original idea to eventual industrial application, not forgetting also the countless failures and bankruptcies – then a theory of 'inevitability' appears ludicrous: it completely ignores the realities of individual achievement, the imaginative insight, sustained effort, and mixture of motives involved.

VI

A possible factor encouraging invention was an 'institutional' one – the patent system – illustrating the role of governmental or parliamentary policy.² This is another field in which economists have been interested, but have reached contradictory conclusions.³ Some have regarded the patent system as vitally important in encouraging invention and enterprise, but others have strongly criticized its monopolistic aspects, its arbitrary unfairness, and the huge amount of litigation to which it has

¹ Usher, *History of Mechanical Inventions*, chap. IV, in which he puts forward a theory, based on 'Gestalt' psychology, half-way between the individualist ('heroic') and social ('inevitability') theories, seeing the process of invention 'as a cumulative synthesis', but involving 'acts of insight' or 'imaginative construction', with chance playing a random role.

² It may be argued, of course, that the development of the patent system was a product of economic and social forces, such as the growth of capitalist enterprise, influencing political policy. A similar factor was the growth of tariff protection, etc., restricting foreign competition and encouraging native manufactures. Parliament also made numerous grants to inventors.

³ See above, pp. 7–8.

given rise. This division of opinion is of long standing: during the Industrial Revolution, there were similar arguments and legal wrangles, but no one has yet made a detailed study of how the system operated in that period.¹ Some general attention has been paid, however, to patent statistics as indicators of technological progress. Witt Bowden was one of the first to emphasize their importance.² He compiled the following figures from the Patent Office publications by Bennett Woodcroft:

Number of Patents Issued

1660-9	31	1730-9	56
1670-9	51	1740-9	82
1680-9	53	1750-9	92
1690-9	102	1760-9	205
1700-9	22	1770-9	294
1710-19	38	1780-9	477
1720-9	89		

Bowden also pointed out that whereas many of the early patents had been granted for vague and often worthless ideas, or merely for bringing a new process into use, by the late eighteenth-century specifications had to be much more precise and patents were only granted for genuine inventions. It is not clear how far inventors were encouraged by the somewhat dubious protection afforded by patents, which were costly to obtain and often challenged in the law courts; many inventions were apparently never patented and secrecy was often preferred. Moreover, patent statistics measure only quantity, not quality, of inventions; the majority were for minor improvements, and many proved abortive.

Ashton put the patent statistics under more revealing analysis.³ He was, of course, well aware of their shortcomings: he stressed that technical change is 'a continuous process', much of it occurring 'behind the scenes' in innumerable small and

¹ For some recent references, however, see above, p. 34 n. 1.

² W. Bowden, *Industrial Society in England towards the End of the Eighteenth Century*, 2nd ed, (New York, 1965), pp. 12-14, 25-30.

³ T. S. Ashton, 'Some Statistics of the Industrial Revolution in Britain', *Manchester School*, vol. XVI (1948), pp. 214-34, extracts from which are reprinted below, chap. 3.

mostly unpatented improvements (a view which has been confirmed by the researches into modern industry of economists such as Salter). Nevertheless, he considered that the patent statistics provide a useful 'rough index of innovation'. He pointed out that they showed not only the expected 'strong upward trend' observed by Bowden and others, but 'also the cyclical variations typical of most economic data'. Peaks in the figures coincided with 'years when the rate of interest was low, and when . . . industry and trade were active', whereas comparatively few were taken out in years of depression, suggesting that invention was motivated by 'the hope of gain, rather than avoiding loss'.¹ Moreover, Ashton discerned a connexion between invention, on the one hand, and the rate of interest and capital investment, on the other, cheap money being a major incentive.² Like many modern economists, he related technological change to the relative prices of the factors of production, capital and labour – the downward trend in interest rates being responsible for the labour-saving bias in most inventions. 'It is at least clear that, whatever the nature of the connecting thread, the inventions were not a force operating more or less casually from outside the system, but were an integral part of the economic process.' Only in recent years, as we have seen, have economists generally come to recognize this fact of economic life.

Ashton had, however, been influenced in his ideas by Sir Arnold Plant's earlier article on the patent system.³ Kuznets had also previously pointed out the significance of patent statistics,⁴ and Merton had carried out similar researches upon them in tracing fluctuations in American industry.⁵ Schmookler's more recent and much more exhaustive investigations have, as we have seen, tended to confirm Ashton's conclusions. Hoffmann, in his investigations into the growth of British industrial production, followed Ashton in tracing 'a close correlation between changes in the number of patents taken

¹ A similar conclusion has been reached by R. S. Sayers, 'The Springs of Technical Progress 1919-39', *Economic Journal*, vol. LX (1950).

² See also Ashton, *The Industrial Revolution*, pp. 9-11.

³ See above, p. 8.

⁴ Kuznets, *Secular Movements*, pp. 54 et seq.

⁵ See above, p. 24 n. 3.

out and changes in the volume of output of the producer-goods industries' during the Industrial Revolution and later:¹ plotted graphically, the curves of patents and output correspond exactly in their 'long waves' (of approximately twenty years). Patents increase with output in booms and decline in slumps, while there is an inverse relationship with bankruptcies: 'as bankruptcies rise, so patents fall'.

Hoffmann has to admit however – and his admission also undermines Ashton's theory – that the date at which a patent is taken out may be of little economic significance, 'since many years may elapse between the patenting of an invention and its practical application; and in any case, the full economic effects of an invention may not be felt for some time after its first application'.² This time-lag between invention (whether patented or not) and development and diffusion has also been admitted by the sociologist Gilfillan, although he fails to appreciate adequately how damaging it is to his theory of social 'inevitability'.³ It is equally damaging to Schmookler's theory of inventions, especially in the case of major ones, and particularly during the Industrial Revolution, when the time-lag was often much longer than in the later period. (One may mention, for example, Darby's coke-smelting process, Huntsman's crucible steel, Kay's fly-shuttle, Paul and Wyatt's roller-spinning, Cartwright's power-loom and combing machine, Watt and Murdock's steam locomotive, and Trevithick's high-pressure engine.) If these inventions were simply products of pressing economic and social forces, why was there such a long time-lag before their widespread application? Surely, if they were sociologically or economically 'determined', 'inevitable', and 'necessary', they should have been brought into

¹ W. Hoffman, *British Industry, 1700–1950* (English trans., 1955), pp. 171–4, 300, and table 54.

² Ashton, in fact, admits this elsewhere: that 'there was often a lag of years between an invention and its application, and it was this last, rather than the discovery itself, that was influenced by such things as a growing shortage of materials or a change in the supply of labour or capital' (*The Industrial Revolution*, p. 92).

³ Gilfillan, *The Sociology of Invention*, chap. V. The time-lag may be 'ten to fifty years'. Ogburn, *Social Change*, similarly recognized these 'cultural lags'. Gilfillan did actually admit (op. cit., p. 152) that in many cases invention may arise from 'an individual, not a social, need, and is an inner aspect'.

widespread use immediately. It has been argued that such inventions were 'before their time', and were not adopted till the market was 'ripe'. But what, then, prompted their original invention? Ashton's more limited and cautious observation – that patents rose and fell with the state of trade, and that this was a factor influencing inventors – is obviously borne out by the statistics, but it fails to distinguish adequately between invention and the later (and economically perhaps more significant) stages of innovation, development, and diffusion, the necessity for distinguishing between which Kuznets has emphasized.

There seems little doubt, however, that – whatever the motives of inventors – innovators or entrepreneurs were certainly very much influenced by economic factors, such as relative factor prices, market possibilities, and profit prospects. Of this there is plentiful evidence in specialized historical studies of particular firms, too well known and numerous to list here.¹ On the other hand, Ashton's emphasis on the role of increasing capital investment, reiterated by Rostow, has tended to be somewhat deflated by recent statistical investigations, which have shown that the proportion of national income invested during the Industrial Revolution did not rise so remarkably as surmised,² and suggest that capital-saving was perhaps more important than hitherto thought (as emphasized by Blaug), that technical advance was not simply a product of capital accumulation, and that the growth of scientific, technical, and managerial knowledge was of considerable significance. In the cotton industry, in particular – Rostow's 'leading sector' – it has been shown that fixed capital formation was a good deal less than has hitherto been thought.³ It was probably only

¹ Good examples are G. Unwin, *Samuel Oldknow and the Arkwrights* (Manchester, 1924); T. S. Ashton, *An Eighteenth-Century Industrialist* (Manchester, 1939); A. Raistrick, *Dynasty of Iron Founders* (1953); R. S. Fitton and A. P. Wadsworth, *The Strutts and the Arkwrights* (Manchester, 1958).

² P. Deane and W. A. Cole, *British Economic Growth 1688–1959* (1964), pp. 263–4. Their estimates indicate a rise in net national capital formation from about 5 per cent at the end of the seventeenth century to only about 6½ per cent in Rostow's 'take-off' period, with a gradual rise thereafter to about 9 per cent in the second quarter of the nineteenth century.

³ Edwards, *op. cit.*, chap. IX; S. D. Chapman, 'Fixed Capital Formation in the British Cotton Industry, 1770–1815', *Economic History Review*, 2nd ser., vol. XXIII, no. 2 (August 1970).

in the second quarter of the nineteenth century, with the large-scale development of machine-tools and mass-production engineering, that technological developments became very capital-intensive and labour-displacing.¹ But how far inventors and innovators were influenced by relative factor prices – whether by the prevailing abundance of relatively cheap capital, as Ashton has argued, or by relatively dear (and troublesome) labour, as others have suggested – and how far by ‘non-economic’ factors, is still far from clear.

In regard to inventors, there is some evidence to support the view that they are just as likely to have been motivated by intellectual or practical curiosity, by a love of ‘tinkering’ or an ‘instinct of contrivance’, or by a non-economic interest in ‘improvement’, as by economic factors.² We have already referred to the differences among economists on this subject.³ Study of some leading figures in the Industrial Revolution indicates that the economic incentive was certainly not the only one, nor necessarily the most powerful. Watt is a particularly good example of this mixture of motives, not only in his improvement of the steam engine, but also in his other scientific-technological interests.⁴ Berthollet, discoverer of chlorine bleaching for textiles, expressly disclaimed, as a scientist, any personal economic interest in its development.⁵ Edmund Cartwright, inventor of the power-loom, was driven mainly by a creative urge and desire to improve: he never stopped inventing, almost to the day of his death, and produced a multifarious list of schemes for industrial and agricultural improvements.⁶ Richard Roberts, inventor of the self-actor mule, was even more prolific, impelled chiefly by a profound interest in mechanical engineering, and apparently lacking in sound business sense.⁷ John Kennedy, the famous cotton-spinner and machine-maker, ‘never pursued business for the

¹ Musson and Robinson, *op. cit.*, chap. XV.

² As suggested by Rossman, Hart, Hatfield, Jewkes, etc.

³ See above, pp. 8, 18–20, and 24–9.

⁴ See above, p. 46, and below, pp. 62, and 108–9.

⁵ Musson and Robinson, *op. cit.*, p. 266.

⁶ Taussig, *op. cit.*, pp. 25–6; Hart, *op. cit.*, p. 656, based on J. Burnley, *The History of Wool and Wool-Combing* (1889), pp. 132–3.

⁷ H. W. Dickinson, ‘Richard Roberts, His Life and Inventions’, *Newcomen Society Transactions*, vol. XXV (1945–7).

sake of money, but for love of improvements in his favourite mechanical pursuits'.¹ William Fairbairn, the great Manchester engineer, was similarly interested in scientific-technical investigations, from motives of intellectual and practical curiosity and desire for renown, rather than for material gain, though they did lead to profitable engineering advances.² Such examples could be multiplied. At the same time, of course, one must beware of motives attributed to inventors by others, especially after their deaths; an impressive quantity of evidence could easily be compiled, suggestive of the stimulus provided by competition and the profit motive. Nor is it correct to regard really able inventors as dilettante amateurs or tinkerers: most of them were actively engaged in industry, though not necessarily in the ones to which their inventions were applicable, and few appear to have been fanciful dreamers.

There is also a good deal of evidence to support the view of Hoselitz that technical knowledge and skills were possibly more important in the success of many industrial entrepreneurs than their financial and commercial abilities.³ Many of the leading manufacturers of the Industrial Revolution came from humble social origins and acquired technical skills and experience while working as practical craftsmen. In the cotton industry, for example, outstanding figures such as McConnel and Kennedy, the Murrays, Ewart, Lee, and many others started in this way, and were notable for their technical capacity and improvements.⁴ The same is true of engineering, as illustrated by Watt, Bramah, Maudslay, Roberts, Fairbairn, Nasmyth, Whitworth, etc. All these men began with very little capital, usually borrowing or securing credit initially from merchants, banks, etc., and it was their technical and managerial skills which formed the basis of their achievements. It is quite misleading to em-

¹ Musson and Robinson, *op. cit.*, p. 100, quoting from Fairbairn's memoir of him.

² W. Pole (ed.), *The Life of Sir William Fairbairn, Bart.* (1877; new ed. 1970, with introduction by A. E. Musson), pp. 464-6, evidence by contemporary associates. See also below, p. 105.

³ See above, p. 32.

⁴ Even Arkwright, above all characterized as a business tycoon, appears to have acquired considerable technical skills and knowledge, which enabled him to develop the water-frame successfully. Owen similarly acquired knowledge of machine-making before becoming a factory manager.

phasize unduly their 'economic' characteristics, though, of course, technical ability alone was not a guarantee of business success. Their 'predominant orientation', to use Hoselitz's phrase, was 'in the direction of productivity, work, and creative integration', as distinct from mercantile personality characteristics.¹

As to the factors which created this 'productive orientation', many explanations are possible. Some might say that it was a national or racial characteristic, somehow produced by geographical, ethnic, and other circumstances (the preponderance of Scots in the above list is not without significance); others might attribute it to a Puritan religious ethic, extolling the virtues of practical hard work, or to other socio-cultural or psychological factors, shaping personality traits favourable to technological innovation;² others might explain it as a development from traditional handicrafts, stimulated by expanding markets.

VII

The role of science in these technological changes is another enigma, which has not, until recently, been investigated with any thoroughness. Indeed, attention has tended to centre more on the preceding 'Scientific Revolution' of the sixteenth and seventeenth centuries, and on whether or not this was motivated by purely intellectual curiosity, or by the 'Puritan ethic' and other social forces, with the related question as to what extent, if any, science was applied in industry.³ Much of the

¹ Hagen, *op. cit.*, pp. 294-309, also stresses the manufacturing background of most of the leading innovators in the Industrial Revolution, though he points out that they 'were not typically poor men'.

² Following Hagen and McClelland. See also T. Burns and S. B. Saul (eds.), *Social Theory and Economic Change* (1967), especially the contributions by Flinn and Hagen. The Dissenters, both English and Scots, certainly played an immensely important role. See below, pp. 61-2.

³ See, for example, A. Wolf, *A History of Science, Technology and Philosophy, in the Sixteenth and Seventeenth Centuries* (1935), and *in the Eighteenth Century* (1938); G. N. Clark, *Science and Social Welfare in the Age of Newton*, 2nd ed. (1949); H. S. Butterfield, *The Origins of Modern Science, 1300-1800* (1949); A. R. Hall, *The Scientific Revolution, 1500-1800* (1954), and *From Galileo to Newton, 1630-1720* (1963); R. Taton (ed.), *The Beginnings of Modern Science from 1500-1800*, trans. by A. J. Pomerans (1944); H. F. Kearney, *Origins of the Scientific Revolution* (1964);

debate in this period, and later in the Industrial Revolution, hinges on what interpretations are given to the words 'science' and 'empiricism', or to 'pure' and 'applied' science. In the present writer's opinion, too strong and unrealistic a distinction is often drawn between them. The fundamental basis of modern science, 'pure' or 'applied', is the 'scientific method' of combining theory or hypothesis with practical experiment: modern science is 'experimental science'. The objects may be different: a 'pure' scientist may well have no utilitarian motives in his researches, whereas an 'applied' scientist is essentially concerned with practical objectives. But the distinction between them is often blurred: a 'pure' scientist may be interested in both furthering knowledge and putting such knowledge to use, while applied science may result in 'feed-back' of theoretically important ideas, as well as experimental information. It must also be remembered that science is not a set of immutable theories or laws, but of hypotheses, which have long been, and are still being altered with the progress of thought and experiment.¹ Science today is far more advanced than it was two or three centuries ago, so that by comparison the science (or 'natural philosophy') of the earlier period may appear crude, elementary, or even empirical. But since the 'Scientific Revolution' of the late medieval and early modern period, 'the scientific method' has been fundamentally the same. Moreover, in a more general way, the influence of the Renaissance, of rationalism and the 'scientific spirit', appears to have opened up vast possibilities of Man's controlling and exploiting his environment 'by reason and experiment'.² This greatly influenced the attitudes of entrepreneurs as well as scientists.

C. Hill, *Intellectual Origins of the English Revolution* (1965); M. Purver, *The Royal Society: Concept and Creation* (1967). The debate has also given rise to many articles: see, for example, those by Merton and Hall referred to above, p. 37. For further references and evidence, see Musson and Robinson, *op. cit.*, chap. 1. See also D. S. Kemsley, 'Religious Influences in the Rise of Modern Science', *Annals of Science*, vol. 24 (1968).

¹ See T. S. Kuhn, *The Structure of Scientific Revolutions*, vol. II, no. 2, of the *International Encyclopedia of Unified Science* (Chicago, 1962). See also Musson and Robinson, *op. cit.*, introduction.

² J. S. Duesenberry, 'Some Aspects of the Theory of Economic Development', *Explorations in Entrepreneurial History*, vol. III, no. 2 (December 1950), pp. 73-4. Duesenberry attaches more importance to this than to the 'Protestant ethic'.

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Francis Bacon and his successors in the Royal Society, such as Boyle and Hooke, were passionately concerned with making science useful, and although the uses of applied science were limited in the seventeenth century, there is little doubt that there were some fruitful interactions between natural philosophy and industry. Usher, referring to the development of the suction pump and steam engine, and to the manufacture of scientific instruments, clocks and watches, has concluded: 'The sixteenth and seventeenth centuries mark the transition from complete empiricism to engineering techniques fully grounded in mathematics and applied science.'¹ Similarly, the late Dr Gibbs, who, before his death was the most knowledgeable authority on the early modern chemical industry, considered that by the first half of the eighteenth century 'the point had been reached where science, and particularly chemistry, could begin to give a lead to several manufactures'.² Professor Rees has also pointed out that, during the late sixteenth and seventeenth centuries, 'a more scientific attitude was being brought to bear on technical processes preparatory to the Revolution of the eighteenth century'.³

These developments continued during the Industrial Revolution, in which the role of science has, until recently, been considerably underestimated, or even ignored. The long-established view, still by no means defunct, has been that the inventors and innovators of that period were mostly practical, often illiterate men, whose achievements were almost entirely products of empiricism, or trial and error, practical experience, and native wit. Nor has this view been confined to economic historians. Some scientists, such as Dr Hardie, have continued to emphasize 'the empirical tradition'. While he admits, somewhat contradictorily, that the Renaissance witnessed 'the

¹ In Singer *et al.*, *History of Technology*, vol. III, p. 344. Professor Hall, however, while acknowledging that there were some significant advances, supports the 'empirical' viewpoint, e.g. 'Engineering and the Scientific Revolution', *Technology and Culture*, vol. II (1961).

² *History of Technology*, vol. III, p. 706. See also his 'Essay Review - Prelude to Chemistry in Industry', *Annals of Science*, vol. 8 (1952), in which he shows 'that applied chemistry in England has a long background history', and that by the early eighteenth century there was fruitful collaboration 'between chemists and the leading manufacturers in many parts of the country'.

³ Rees, *op. cit.*, foreword.

emergence of the scientific method and modern technologies', he considers that empiricism predominated in the chemical industry as late as the end of the nineteenth century.¹ It is now becoming clear, however, that this traditional view is inadequately based on historical research and that applied science played a considerably more important role than has been generally realized.

Bowden was one of the first historians to recognize this,² pointing out the growing spirit of rational inquiry, the increasing interest in 'experimental and applied science', expressed at the national level in the Royal Society and Society of Arts, and locally in such bodies as the Manchester Literary and Philosophical Society, the development of education, particularly in new colleges and schools where more attention was paid to 'the useful arts and sciences', and the growing numbers of periodicals, encyclopedias, dictionaries of arts and sciences, etc., containing scientific and technical information. Ashton also appreciated the significance of these developments:³

The stream of English scientific thought, issuing from the teaching of Francis Bacon, and enlarged by the genius of Boyle and Newton, was one of the main tributaries of the industrial revolution. . . . Physicists and chemists, such as Franklin, Black, Priestley, Dalton, and Davy, were in intimate contact with the leading figures in British industry; there was much coming and going between the laboratory and the workshop, and men like James Watt, Josiah Wedgwood, William Reynolds, and James Keir were at home in the one as in the other. The names of engineers, ironmasters, industrial chemists, and instrument-makers on the list of Fellows of the Royal Society show how close were the relations between science and practice at this time.

¹ D. W. F. Hardie, 'The Empirical Tradition in Chemical Technology', the first Davis-Swindin Memorial Lecture, Loughborough, 1962. Dr Hardie, however, appears almost to equate 'applied science' with 'empiricism', in distinguishing it from 'pure' science. Moreover, some of his own researches hardly support this view. See below, chap. 7, for his article on the Macintoshes and the early chemical industry.

² Bowden, *op. cit.*, chap. 1.

³ T. S. Ashton, *The Industrial Revolution 1760-1830* (1948), pp. 16, 19-21.

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More recent studies have confirmed the importance of the links between science and industry in that period.¹ The general background of knowledge, education, and training, now being stressed by economists in regard to modern technological progress, has been shown to have been vitally significant during the Industrial Revolution. The work of educational historians, especially Dr Hans,² has been followed by further research which has demonstrated in considerable detail the profusion of schools, colleges, libraries, books, periodicals, encyclopedias, philosophical societies, itinerant lecturers, etc. during the late eighteenth and early nineteenth centuries – with growing emphasis on mathematics, science, and technology – and how much more widespread was scientific-technical education, both formal and informal, than has hitherto been realized.³ It is clear that the diffusion of such knowledge took place at all levels, ranging from the highest scientific advances down to elementary instruction in mathematics, mechanics, etc. for humble artisans and craftsmen. In the Royal Society and Society of Arts, scientifically-minded industrialists mingled with eminent philosophers; there was a similar community of interests in local societies such as the Birmingham Lunar Society,⁴ the Manchester Literary and Philosophical Society, and innumerable similar, but less famous, bodies established in

¹ See, for example, in addition to the previously cited works by Musson and Robinson, A. and N. L. Clow, *The Chemical Revolution* (1952); R. E. Schofield, *The Lunar Society of Birmingham* (1963); S. Pollard, *The Genesis of Modern Management* (1965), chap. IV.

² N. Hans, *New Trends in Eighteenth-Century Education* (1951).

³ Musson and Robinson, op. cit., especially chap. III. Professor D. C. Coleman has, with evident prejudice, accused us of exaggerated pretensions to originality, referring particularly to Ashton's earlier work (*Economic History Review*, 2nd ser., vol. XXIII, no. 3, December 1970, pp. 575–6). In fact, however, we explicitly acknowledged that we were 'not, of course, the first to explore this territory' (op. cit., p. 7); we fully acknowledged Ashton's prior discernment (pp. vii and 88), though he devoted only a few paragraphs to scientific-technological aspects of the Industrial Revolution; and we clearly recognized the earlier work of other scholars, such as Wolf, Clark, Singer, Hall, Clow, Schofield, Hans, etc. (see pp. 7, 87–8, and innumerable other references). At the same time we can justifiably claim to have produced a great deal of entirely new material, and to have brought about 'some modification of the traditional view of the Industrial Revolution' (p. 189), as other well-informed reviewers, such as Landes, Schofield, Pollard, etc., have fully recognized.

⁴ See below, chap. 5, by Professor Schofield.

provincial towns throughout the country; while ordinary millwrights, builders, weavers, etc. sometimes belonged to the smaller local clubs, foreshadowing the later mechanics' institutes. Some leading industrialists were educated either at the universities, especially in Scotland, or in Dissenting academies, while many others achieved remarkable feats of 'self-education', with the aid of books and libraries, and courses by itinerant lecturers, whose audiences, often very large and varied, could see numerous practical models of machinery, etc. The importance of an educated, knowledgeable, and experienced class of managers has also been demonstrated: the 'managerial revolution' is not as recent a phenomenon as has usually been imagined.¹ There seems, indeed, little doubt that in the Industrial Revolution, as in modern economic growth, the importance of the 'capital stock of knowledge', of 'human capital' and 'investment in human beings', in improved education and training, has hitherto been seriously neglected, together with associated developments in applied science and technology. These developments were important not only in major applications of scientific knowledge to industrial problems, but also less dramatically, though more extensively, in innumerable piecemeal improvements resulting from the improving literacy and numeracy of the mass of entrepreneurs, managers, and skilled workers, among whom technical knowledge was being diffused through an increasing multiplicity of channels.

In all these educational developments, as well as in the related industrial changes, there is no doubt that Nonconformists, especially certain groups such as the Quakers and Unitarians, played a major role, out of all proportion to their numbers in the population. The emphasis by Merton and others on the scientific-technological significance of the 'Puritan ethic' in the earlier period receives considerable support from researches into the Industrial Revolution. Raistrick and others have revealed the multifarious activities of Nonconformists in industry, trade, and banking;² the Clows have shown the links

¹ Pollard, *op. cit.*

² A. Raistrick, *Quakers in Science and Industry* (1950); I. Grubb, *Quakerism and Industry before 1800* (1930); E. D. Bebb, *Nonconformity and Social and Economic Life, 1660-1800* (1934); R. V. Holt, *The Unitarian Contribution to Social Progress in England*, 2nd ed. (1952).

between Scottish universities, Dissenting academies, and scientific-industrial innovations;¹ McClelland has also emphasized the superiority of Dissenting education and childhood training in 'achievement motivation', leading to entrepreneurial enterprise and technological innovation;² and Hagen has similarly stressed the psychological drives of these 'alienated' or 'disparaged' groups, who were impelled to strive for improved status in scientific or industrial achievement.³

There is no doubt of the immense importance of these socio-psychological forces, and of the associated educational-scientific-technological factors, in the Industrial Revolution, which cannot, therefore, any longer be explained simply in economic terms of supply and demand. There is now plentiful evidence of fruitful collaboration between industrialists and scientists, and of applications of science in various fields. A few examples only must suffice here. The early steam engine, it is now clear, owed much to the scientific researches of philosophers such as Boyle and Huygens in the seventeenth century and was not just a product of empiricists such as Savery and Newcomen.⁴ Watt's improvements were similarly based on scientific knowledge and careful experimental researches, while he benefited considerably from contact not only with Professor Black, discoverer of the principle of latent heat, but also with other scientists at Glasgow University, particularly Anderson and Robison.⁵ Trevithick was likewise aided in his development of the high-pressure engine by scientific advice from Davies Gilbert (or Giddy).⁶ Water-wheels, moreover, which have generally been regarded, by comparison with steam engines, as a part of 'traditional' technology,⁷ were, in fact, enormously improved in efficiency and power as a result of practical-scientific experiments, particularly by John

¹ Clow, *op. cit.*, *passim*. On the role of the Dissenting academies, see also Irene Parker, *Dissenting Academies in England* (1914), and H. McLachlan, *English Education under the Test Acts* (Manchester, 1931).

² McClelland, *op. cit.*, pp. 46-57, 132-49.

³ Hagen, *op. cit.*, chap. XIII.

⁴ See the introduction by A. E. Musson to H. W. Dickinson, *Short History of the Steam Engine*, 2nd ed. (1963).

⁵ Musson and Robinson, *Science and Technology*, pp. 79-80, and *James Watt and the Steam Revolution*. See below, pp. 108-9.

⁶ Musson and Robinson, *Science and Technology*, pp. 80-1. See below, p. 109.

⁷ See, for example, Lilley, *Technical Progress*, pp. 8-9.

Smeaton, and by other scientifically-minded hydraulic engineers including Hewes, Rennie, and Fairbairn.¹ As Cardwell has demonstrated, there were also close links between science and technology in the development of water as well as of steam power.² Even the ordinary millwright, as Fairbairn emphasized, usually had a good knowledge of arithmetic, geometry, machine-drawing, and mechanics, as well as wide constructional experience,³ while leading engineers such as those mentioned were generally as able in 'practical science' as the philosophers with whom they associated, sometimes more so.⁴

Similarly, in applied chemistry there is accumulating evidence not only from the chemical manufactures themselves, producing acids, alkalis, etc.,⁵ but also from allied trades such as glass, pottery, and soap. Josiah Wedgwood, for instance, has been revealed as an able research chemist, employing Alexander Chisholm, a Scottish graduate and previously laboratory assistant to the then-famous industrial chemist Dr William Lewis.⁶ James Keir, like Wedgwood a Fellow of the Royal Society and member of the philosophical society meeting at the Chapter Coffee House in London, as well as of the Birmingham Lunar Society, was another product of the Scottish universities, who not only applied his chemical knowledge in the manufacture of glass, soap, etc., at his famous works at Tipton in Staffordshire, but also wrote a *Dictionary of Chemistry* (1789), in which he referred to the 'diffusion of a general knowledge, and of a taste for science' as being 'the characteristic feature of the present age'.⁷ In textile chemistry, the introduc-

¹ Musson and Robinson, *Science and Technology*, pp. 67-71, 73-4, 76, 98, 445-6, 481-2.

² D. S. L. Cardwell, 'Power Technologies and the Advancement of Science, 1700-1825', *Technology and Culture*, vol. VI, no. 2 (Spring 1965), and 'Some Factors in the Early Development of the Concepts of Power, Work and Energy', *British Journal for the History of Science*, vol. III (1966-7).

³ W. Fairbairn, *Treatise on Mills and Millwork* (1861-3), vol. 1, pp. v-vi. See below, p. 100.

⁴ As Professor Robison acknowledged in the case of Watt.

⁵ See below, chaps. 6, 7, and 8, by Clow, Hardie, and Gibbs.

⁶ R. E. Schofield, 'Josiah Wedgwood, Industrial Chemist', *Chymia*, vol. 5 (1959); Musson and Robinson, op. cit., p. 78. See below, p. 106.

⁷ Musson and Robinson, op. cit., *passim*; J. L. Moilliet, 'James Keir of the Lunar Society', *Notes and Records of the Royal Society of London*, vol. 22, nos. 1 and 2 (September 1967).

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tion of vitriol sours and chlorine bleach not only resulted from researches by scientists such as Home, Scheele, and Berthollet, but was also considerably aided by industrial chemists in its development and diffusion. In Manchester, enterprising manufacturers such as Henry, Cooper, Taylor, and others who were outstanding in their chemical knowledge, played leading roles in the scientific-technical advances made in bleaching, dyeing, and calico-printing.¹ In Scotland, Charles Macintosh, who had studied at Glasgow and Edinburgh universities and was 'equipped with the best training that the "pure" chemistry of his day afforded', played a similar pioneering role in textile-chemical manufactures as well as in later founding the rubber industry in Manchester.²

In Manchester, as in Birmingham, many of the leading cotton-spinners and engineers, including Lee, Ewart, Kennedy, Hewes, Fairbairn, and Nasmyth, were well versed in theory as well as practice; they collaborated with men of science, and put their theoretical knowledge to industrial use in the construction of improved machinery.³ Similar links between science and industry have been discovered in other industries and in other areas. Thus we find Thomas Telford, the famous road and bridge builder, going 'chemistry mad', avidly reading the works of Black, Fourcroy, and other chemists, to find out more about building materials, cramming mechanics, hydrostatics, and other scientific information into his working notebook, and collaborating with scientists in practical scientific publications and experiments;⁴ John Rennie, the famous London engineer, another Scottish graduate, applying his scientific knowledge in construction of water-wheels, mills, and bridges;⁵ John Southern, Boulton and Watt's manager, experimenting on heavy machinery to elucidate the theory of mechanics, reading papers on the subject to the Royal Society

¹ Musson and Robinson, *op. cit.*, especially chaps. VII, VIII, and IX.

² D. W. F. Hardie, 'The Macintoshes and the Origins of the Chemical Industry', *Chemistry and Industry* (June 1952), reproduced below, chap. 7. Musson and Robinson, *op. cit.*, pp. 293-5, 322-6; Clow, *op. cit.*, *passim*.

³ Musson and Robinson, *op. cit.*, *passim*.

⁴ *Ibid.*, pp. 74-6. See below, pp. 102-4.

⁵ C. T. G. Boucher, *John Rennie 1761-1821* (Manchester, 1963); Musson and Robinson, *op. cit.*, pp. 76-7. See below, pp. 104-5.

(like Smeaton and other engineers), and holding his own with university professors in applied mathematics (in bridge-building, for example);¹ David Mushet, the famous Scottish ironmaster, writing a remarkable series of papers in the *Philosophical Magazine*, based on brilliant applications of chemistry and mineralogy to iron smelting and forging;² John Marshall, the great Leeds flax-spinner, carrying out prolonged experiments in chlorine bleaching, with the aid of the latest chemical publications and skilled French chemists.³ So one could go on, almost endlessly.

At the same time, of course, traditional crafts and skills continued to be extremely important. Many trades and processes, in fact, remained largely if not entirely empirical, and even in those where science was applied, it had to be combined with much empirical trial-and-error. It is interesting to find one of the leading textile engineers in Manchester unconsciously confirming from his own practical experience the truth of Adam Smith's observation on the importance simply of subdivision and specialization of labour.⁴ Where scientific theory was utilized, it was often rudimentary. For this and other reasons some scholars, such as Professor Mathias and Dr Gillispie, continue to be somewhat sceptical.⁵ Gillispie, for instance, to some extent shares Hardie's view as to the long-continued importance of empiricism in the manufacture of soda.⁶ He points out that the chemical reactions in the Leblanc process were not properly understood scientifically until nearly a century later, and considers, therefore, that Leblanc cannot be said to have applied chemical theory in inventing the process. But Gillispie does not take proper account of the evolution of scientific theory: it would seem that, by his view of science, nothing can be called scientific until complete and pure scientific truth has been established. (Has it even yet?)

¹ Musson and Robinson, *op. cit.*, p. 172 n. 5.

² *Ibid.*, pp. 184-5.

³ *Ibid.*, pp. 329-32; W. G. Rimmer, *Marshall's of Leeds, Flax-Spinners. 1788-1886* (1960), pp. 50-3. See below, pp. 106-7.

⁴ Musson and Robinson, *op. cit.*, p. 64.

⁵ See below, chaps. 1 and 4.

⁶ In addition to chap. 4, below, see C. C. Gillispie, 'The Discovery of the Leblanc Process', *Isis*, vol. 48 (1957).

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Many chemical theories of the late eighteenth century may appear to some modern scientific minds as crude and 'wrong', but Leblanc, Berthollet, and others were among the leading scientists of their day, and were applying the best scientific knowledge then available, in fields such as soda manufacture and bleaching. What is significant and truly scientific is their rational, controlled experimental procedure.

S. D. Chapman, reviewing *Science and Technology*, accuses us of almost entirely ignoring empirical techniques and of exaggerating the importance of scientific technology; he quotes from the millwright, John Sutcliffe, to indicate the inadequacies of contemporary scientific theory.¹ Regrettably, however, he does not appear to have read our book very carefully, otherwise he could hardly have failed to notice the innumerable passages where, in fact, we emphasize 'the continued importance of practical craftsmanship'; indeed we point out that such empiricism was of 'immense and probably predominant importance'.² We repeatedly stress that

we do not wish to exaggerate the extent to which natural philosophy contributed to 'arts and manufactures' in the Industrial Revolution. This period was, of course, a transitional one, and traditional handicrafts, with their rule-of-thumb procedures, proved remarkably long-lasting in many industries, while even in those industries which were being most rapidly 'revolutionized' it is clear that practical empiricism was largely responsible for technical advance. Several of our studies, in engineering and dyeing, for example, ... demonstrate its continued importance.³

We ourselves quoted contemporary opinion as to the limited applicability and sometimes erroneous principles of eighteenth-

¹ *Textile History*, vol. 1, no. 3 (December 1970), pp. 373-5.

² *Science and Technology*, pp. vii-viii and 65. See also pp. 7-8, 27, 38, 49-50, 56, 67, 81-4, 189, etc. It is remarkable that in a journal concerned specifically with textiles, this reviewer should make no reference whatever either to our extensive researches into early textile engineering in Lancashire, with its heavy reliance on traditional crafts, or (on the other hand) to the abundant evidence we have produced on the applications of chemistry in bleaching and dyeing (in which, however, empirical techniques also remained extremely important).

³ *Ibid.*, pp. 7-8. See also below, pp. 97-9, and 109-13.

century 'natural philosophy'.¹ In fact, we made the very point which Chapman imagines he has discovered, that philosophers such as Desaguliers and Banks were wrong in some of their theorizing on water-wheels, etc.,² and we also demonstrated repeatedly the importance of traditional millwrights (including Sutcliffe, incidentally).³ On the other hand, what Chapman also overlooks is the fact that the three most outstanding hydraulic engineers of the age – Smeaton, Hewes, and Fairbairn – were all notable for their applied scientific achievements, and that many ordinary millwrights acquired knowledge of mathematics, mechanics, etc. for utilitarian purposes.

Cardwell has similarly shown how engineering theory gradually evolved, how it was applied to practical problems, and how practical experiment modified theory.⁴ He also demonstrates that in mechanical engineering, as in chemistry, French scientists led the way. This French scientific predominance (though perhaps exaggerated) has led to the questioning of the significance of applied science in Britain during the Industrial Revolution, for if science were of fundamental importance to industrial development, one would have expected France to have achieved industrial leadership. This, however, is perhaps taking too narrowly national a view of the interconnexions between science and technology. Scientific ideas were not confined within national boundaries, as Sir Gavin de Beer has emphasized,⁵ and there were many fruitful contacts not only between British, French, and other scientists, but also between the latter and British industrialists, who in some cases met or corresponded with foreign philosophers, and in many more cases studied their published works (either in the foreign language or in translations, which were very quickly made), and applied this knowledge in engineering, chemical manufactures, bleaching and dyeing, etc. While France was ahead in many branches of scientific theory, the reverse appears to have been the case in applied or industrial science, and practical-

¹ See, for example, *ibid.*, pp. 82-4. See also below, pp. 110-13.

² *Ibid.*, pp. 37, 73 n. 7, 83-4, and 107 n. 9.

³ See, for example, *ibid.*, pp. 443-5, in addition to previous references. See also below, pp. 97-9.

⁴ See above, p. 63 n.2

⁵ Sir Gavin de Beer, *The Sciences were never at War* (1960).

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scientific knowledge seems to have been more widely and deeply diffused in Britain than in France. At the same time, of course, Britain had certain economic advantages over France, in natural resources (especially coal) and wider overseas trade and empire, as well as institutional advantages such as freedom from internal tolls and fewer Government restrictions, together with social advantages such as a more developed and enterprising middle class and greater social mobility.¹

In the end, then, one has to recognize the existence of a multiplicity of interacting factors – economic, social, political, and psychological, as well as scientific and technical – among which there is not much possibility of indicating preponderance. Certainly there is no possibility whatever of *measuring* the percentage contribution of science and technology to economic growth in this period, either at the national level or even at that of the individual firm. A ‘disaggregated’, qualitative approach is the only feasible one, while bearing in mind the general concepts now emerging from economic, sociological, and psychological analysis in this field.²

¹ See F. Crouzet, ‘Angleterre et France au XVIII^e Siècle. Essai d’Analyse Comparée de Deux Croissances Economiques’, *Annales* (1966), trans. in Hartwell, *op. cit.*

² Professor Coleman has made some absurdly distorting comments on what he alleges to be our exaggerated claims to have reinterpreted the whole Industrial Revolution in scientific-technological terms, without reference to economic and social factors (*Economic History Review*, vol. XXIII, no. 3, December 1970, pp. 575–6). If, however, he had read our Preface and Introduction, instead of quoting misleadingly from the publisher’s ‘blurb’, he would have found our claims to be modest and cautious, combined with a clear warning statement, strongly emphasized in italics (Musson and Robinson, *op. cit.*, p. 8), ‘*that scientific and technological factors were by no means entirely responsible for the Industrial Revolution, and that they were closely related to the economic and social changes of the time. But we do not propose in this book to deal explicitly with these complex interrelationships.*’ While we recognized the importance of population growth, trade expansion, transport developments, capital investment, etc., we emphasized that our researches were more narrowly concerned with producing new evidence on the scientific-technological changes.

I Who Unbound Prometheus? *Science and Technical Change, 1600–1800*¹

PETER MATHIAS

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I

An economic historian is interested in science not for its own sake (which for a historian of science is doubtless the only academically respectable way of looking at it) but for his own utilitarian purposes. He asks the questions: how was science related to technology at this time? how far did scientific change influence the process of technological change? to what extent was the Industrial Revolution associated with scientific advance? Taking the very long view from medieval times to the present day is to see a dramatic change in these relationships. Broadly, we may postulate the earlier position as a context where empirical discoveries and the development of industrial processes in such industries as metals, textiles, brewing, dyeing took place and advanced without being directly consequential upon knowledge of fundamental scientific relationships in the materials concerned. The chemistry of what happened inside a blast furnace was not known until the mid-decades of the nineteenth century. The secrets of fermentation were first revealed by Pasteur. There might be close links between science and technology in other ways, but this was none the less a world very different from our own where industrial advance becomes more directly consequential upon the advancing frontier of scientific and technological knowledge, with a developing institutional relationship between science and industry to consolidate the connexion.

¹ © 1972 Cambridge University Press. An early version of this article appeared in the *Yorkshire Bulletin of Economic and Social Research*, vol. XXI (I), 1969.

For the pivotal period of the seventeenth and eighteenth centuries, however, which saw dramatic advances in both scientific knowledge and industrial techniques, varying answers have been offered to these questions by economic historians and scholars generalizing about the relationships from the side of science. Professor A. R. Hall summed up for the earlier period 1660–1760 ‘... we have not much reason to believe that in the early stages, at any rate, learning or literacy had anything to do with it [technological change]; on the contrary, it seems likely that virtually all the techniques of civilization up to a couple of hundred years ago were the work of men as uneducated as they were anonymous’.¹ Sir Eric Ashby concludes for the period 1760–1860: ‘There were a few “cultivators of science” (as they were called) engaged in research, but their work was not regarded as having much bearing on education and still less on technology. There was practically no exchange of ideas between the scientists and the designers of industrial processes.’² Professor Landes is equally firm in this opinion to as late as 1850.³ A. P. Usher is in the same tradition.⁴

Equally forthright assertions crowd the other side of the stage. ‘The stream of English scientific thought,’ writes Professor Ashton, ‘was one of the main tributaries of the Industrial Revolution. . . . The names of engineers, iron-masters, industrial chemists, and instrument makers on the list of Fellows of the Royal Society show how close were the relations between science and practice at this time.’⁵ Professor Rostow, considering the whole sweep of economic change in western Europe, gives the two essential features of post-medieval Europe as ‘the discovery and re-discovery of regions beyond western Europe, and the initially slow but then accelerating development of modern scientific knowledge and

¹ A. R. Hall, *The Historical Relations of Science and Technology* (inaugural lecture) (London, 1963). J. D. Bernal, *Science in History* (London, 1954), pp. 345–6, 352, 354–5, 365–6, 370 argues in a similar vein.

² Sir E. Ashby, *Technology and the Academics* (London, 1958), pp. 50–1.

³ D. Landes in *Cambridge Economic History of Europe*, vol. VI, pt. 1 (Cambridge, 1965), pp. 333, 343, 550–1; also *The Unbound Prometheus* (1969), pp. 104, 113–4, 323.

⁴ A. P. Usher, *A History of Mechanical Invention* (Boston, 1954 ed.) contains very little reference to the role of science in this period.

⁵ T. S. Ashton, *The Industrial Revolution* (London, 1948), pp. 15–16.

attitudes'.¹ When considering the essential propensities for economic growth (relationships that he does not specifically limit in time or place) the first two on his list are: 'the propensity to develop fundamental science and to apply science to economic ends'.² For the English case A. E. Musson and E. Robinson have recently sought to demonstrate how extensive the linkages were between innovation and science, between scientists and entrepreneurs.³ They see this co-operation assisting England to 'retain that scientific lead over the Continent upon which she established her industrial supremacy'.⁴ The Lunar Society, now documented at great length, has been called 'a pilot project or advance guard of the Industrial Revolution' on the argument that 'strong currents of scientific research underlie critical parts of this movement'.⁵

Many more such summary assertions could be deployed on either side. It seems likely that, as historians explore more systematically and in more local detail the development of different branches of the chemical industry and other industrial processes involving chemistry (following up the seminal work on the Chemical Revolution by A. and A. N. Clow, published in 1952); as they find out more about the various local societies of gentlemen meeting in small towns up and down the country in the eighteenth century on the lines of the Lunar Society of Birmingham, the balance will tip heavily towards the positive equation. This theme is captured in the remark: '. . . science is the mother of invention; finance is its father'.⁶

The question, therefore, invites discussion. The arguments, however, should be prefaced with one or two comments.

¹ W. W. Rostow, *The Stages of Economic Growth* (Cambridge, 1960), p. 31.

² W. W. Rostow, *The Process of Economic Growth* (New York, 1952), p. 23.

³ A. E. Musson and E. Robinson, 'Science and Industry in the late Eighteenth Century', *Economic History Review*, vol. XIII (1960), pp. 222-4; *Science and Technology in the Industrial Revolution* (Manchester, 1969).

⁴ E. Robinson, 'The Lunar Society and the Improvement of Scientific Instruments II', *Annals of Science*, vol. XIII (1957).

⁵ R. E. Schofield, *The Lunar Society of Birmingham* (Oxford, 1963), pp. 410, 437. The argument is summed up on pp. 436-40. See also the special issue of the *University of Birmingham Historical Journal* (vol. XI, no. 1, 1967) devoted to the Lunar Society, particularly the articles by E. Robinson, M. J. Wise and R. E. Schofield; E. Robinson, 'The Lunar Society, Its Membership and Organization', *Newcomen Society Transactions*, vol. XXXV (1962-3).

⁶ T. H. Marshall, *James Watt* (London, 1925), p. 84.

Without the assumption that a simple, linear, cause-and-effect relationship exists between phenomena like scientific knowledge and innovations in technique, multi-dimensional historical developments such as the Renaissance or the French Revolution or the Fall of the Roman Empire or the Industrial Revolution, cease to be analysable in terms of single-cause, single-variable phenomena. In the last analysis, quantification of contributory causes of them is impossible, given the intractable nature of the evidence and the subtlety of the interrelationships, direct and indirect, involved. Therefore, no intellectually satisfying proof becomes possible that one answer is demonstrably 'correct' in a scientifically provable way. Quantification does not offer any obvious solution either. One might hope that, taking a defined population of innovations, it would be possible to determine the percentage which depended upon scientific knowledge, or to allocate degrees of such dependence upon some quantified scale. But establishing the criteria of such a scale would be subjective enough, while yet greater discretion would remain in allocating most innovations to the different boxes. Moreover, innovations form a most heterogeneous collection, differing very greatly in relative importance. Bringing qualitative considerations into the argument would imply further discretionary allocation of innovations into a scale of importance so that the degree of dependence of innovations upon scientific knowledge could be construed against some norm of economic significance. Were the scientifically-orientated innovations in the 'population' more, or less, important than their arithmetical proportion suggests?

The question of the *strategic* importance of innovations raises a further issue. For example, despite the percentage of total technical change subject to the linkage with science being small, a strategic blockage on a narrow front at the frontier of technical possibilities might hold up innovation in a wider span behind it. One strategic science-linked innovation could make possible a large number of empirically-based innovations which were, to a degree, dependent upon that initial advance, and vice versa. Moreover, it is impossible to demonstrate the potential quantitative importance of this by being able to

indicate what would have happened if an absolute blockage at the frontier had occurred without substitute arrangements bypassing the obstruction. Perhaps detailed analysis can be applied in the micro-study of particular innovations (carefully chosen), but it is difficult to see how a quantified assessment can be made for the wide sweep of innovations under discussion here. History is a depressingly inexact science as economists – let alone natural scientists – discover to their frustration.

Conclusions in this field are also much influenced by methodological or definitional problems. Controversies on such general themes characteristically sink under the weight of semantic disagreement and pleas for more systematic research. What do we include in (or exclude from) the concept 'innovation'? Were the activities of these seventeenth and eighteenth-century people, properly speaking 'scientific'? Was it *real* science, identified by some later, designated, objective norm – in the 'Baconian' or mechanistic tradition – or was it bogus, mistaken, irrational – and following a magical, alchemical, or Hermetical tradition?¹ How much, for example, can one claim for Jethro Tull, eagerly pursuing 'scientific' technique in agriculture on the assumption that air was the greatest of all manures and that the fertility of soil consequently varied in direct correlation with the amount of ploughing and pulverizing that it received, to the exclusion of all else. Bogus science, quasi science, mistaken science, amateur science which was so very prominent in the seventeenth and eighteenth centuries, particularly in the field of chemistry (where the direct linkage between science and industry are probably most diffused) does raise interesting issues. Does one judge these practitioners by their intentions, their motivations, or by their results, however mistaken their assumptions, looked at *ex post facto* with hindsight? Arguments about distinctions between 'pure' and 'applied' science relate to these controversies, for the seventeenth and eighteenth centuries no less than the nineteenth and twentieth.²

¹ P. Rattansi, 'The Social Interpretation of Seventeenth Century Science', in P. Mathias (ed.), *Science and Society, 1600–1900* (Cambridge, 1972).

² R. K. Merton, 'Science, Technology and Society in Seventeenth-Century England', *Osiris*, vol. IV (1938); A. R. Hall, 'Merton revisited: Science and

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This paper will first explore the positive case and then consider its qualifications.¹ The key question to be answered is not what examples can be found of links between science and industry in the period but rather how important relative to other sources of impetus was scientific knowledge to industrial progress? Can it be judged 'an engine of growth' for innovation, or a pre-condition? In short, how extensive were the linkages, how strategic, how direct?

II

If economic history is written from the evidence of intention, of aspiration and endeavour, rather than the evidence of results (which is often less accessible), then these connexions appear very intimate indeed. In the first place, a very large number of persons – scientists, industrialists, publicists, and government servants – said loudly in the seventeenth century and have gone on saying ever since then, even more loudly, that the linkage was important and ought to be encouraged. For most of the 'professional' scientists of the Restoration the improvement of techniques in the material world, science in the service of a technological utopia, was a subordinate quest, a relatively low priority. But, even so, many such as Robert Boyle, were active on both sides of the watershed between searching for knowledge and applying knowledge to practice, and certainly acknowledged that *one* of the roles of science was to help where it could. Boyle's *Usefulness of Natural Philosophy* (1664) was a systematic survey of the methods then used in industry and

Society in the Seventeenth Century', *History of Science*, vol. II (1963); C. C. Gillespie, 'The Natural History of Industry', *Isis*, vol. XLVIII (1957).

¹ An equivalent, and connected debate, which will not be considered here, is in progress over these relationships, and that of religion, in the seventeenth century. See H. K. Kearney, *Origins of the Scientific Revolution* (London, 1964); C. Hill, *The Intellectual Origins of the English Revolution* (Oxford, 1965), C. Hill, H. F. Kearney, and T. K. Rabb in *Past and Present*, vols. XXVIII (p. 81), XXIX (p. 88), XXXI (pp. 104, 111), XXXII (p. 110). The thesis was formulated by R. K. Merton, 'Science, Technology, and Society in Seventeenth-Century England', *Osiris* (1938). See also S. F. Mason, 'The Scientific Revolution and the Protestant Revolution', *Annals of Science*, vol. IX (1953); D. S. Kemsley, 'Religious Influences in the Rise of Modern Science', *Annals of Science*, vol. XXIV (1968). This, in turn, is an extension of the much older debate about the links between Protestantism and Capitalism, from Max Weber.

of the ways in which science was improving them and would continue to do so. 'These [mechanical] arts', he wrote, 'ought to be looked upon as really belonging to the history of nature in its full and due extent.'¹ 'There is much real benefit to be learned [from mathematical or philosophical inquiries],' wrote Dr J. Wilkins in 1648, 'particularly for such gentlemen as employ their estates in those chargeable adventures of Drayning, Mines, Cole-pits, etc. . . . And also for such *common artificers* as are well skilled in the *practise* of the arts. . . .'² Boyle was himself active particularly in investigating the techniques of mining, assaying, and agriculture. In evidence of intention, if not of result, John Richardson changed the title of his book on *Philosophical Principles of the Art of Brewing*, much taken up by the largest brewers in London, to *Philosophical Principles of the Science of Brewing*.³ R. Shannon's more empirically titled work *Practical Treatise on Brewing* was primarily a plea that brewers and distillers should profit from contact with 'men of reflection acquainted with first principles who have more methodically considered the subject'. 'Chemistry', he remarked, 'is as much the basis of arts and manufactures, as mathematics is the fundamental principle of mechanics.'⁴ They were echoing a traditional sentiment which continued to reverberate until scientific discoveries with major implications for technology in the industry really were made by Pasteur and others in the mid-nineteenth century.

Two eminent Victorians, out of many, may be quoted to show the canon during the nineteenth century. Charles Babbage, writing *On the Economy of Machinery and Manufactures* (1835) concluded: '. . . it is impossible not to perceive that the arts and manufactures of the country are intimately connected with the progress of the severer sciences; and that, as we advance in the career of improvement, every step requires, for its success, that this connexion should be rendered more

¹ Cf. A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge, 1952), p. 3; also G. N. Clark, *Science and Social Welfare in the Age of Newton* (Oxford, 1937), p. 14.

² J. Wilkins, *Mathematicall Magick* . . . (London, 1649), p. vi.

³ J. Richardson, *Philosophical Principles of the Art of Brewing* (Hull, 1788); *Philosophical Principles of the Science of Brewing* (Hull, 1798).

⁴ R. Shannon, *Practical Treatise on Brewing* (1804), pp. 48-9.

intimate.’¹ Dr Lyon Playfair, the forward-looking Scot who helped to organize the Great Exhibition of 1851 wrote, with justly famous perception: ‘Raw material, formerly our capital advantage over other nations, is gradually being equalized in price, and made available to all by improvements in locomotion, and Industry must in future be supported, not by a competition of local advantages, but by a competition of intellects.’² The assertion of the linkage has formed a continuum; and still does.

Apart from such aspirations look also at what endeavours actually took place. The State actively sought to press scientists into utilitarian endeavour. A long list of instances can be drawn up. Typical examples are ballistics and navigation (improvements in cartography, scientific instruments, astronomy, mathematical tables, accurate time-keeping lay behind this). Much medical experimentation went on sponsored by the Admiralty, facing particular problems of maintaining efficiency in fleets, long on foreign station, from scurvy and other diseases. Standardization in production, in dockyards, of interchangeable parts, exact measurement techniques were much encouraged by this. Industrial and scientific skills likely to be useful in war received particular attention. More widely, national rivalries became important in the seventeenth century for stimulating inventions in many industries where there was most technical progress – export industries, sugar refining, distilling, glass blowing, silk, tobacco, book printing, paper making, and others.³

The Royal Society in England of 1662, as the French *Académie* of 1666, personified such state patronage (although in England with virtually no public resources) for utilitarian ends, an intention explicitly stated in its first charter. The draft preamble of the statutes of the Royal Society ran: ‘The business of the Royal Society is: to improve the knowledge of natural

¹ C. Babbage, *On the Economy of Manufactures* (London, 1835), p. 379, para. 453.

² Lyon Playfair, *Lectures on the Results of the Great Exhibition of 1851* (London, 1852). Royal Commissions joined the chorus in 1864 with the publication of the Taunton Commission report on technical and scientific education. Mark Pattison made the same plea in *Suggestions on Academical Organization* . . . (Edinburgh, 1868).

³ G. N. Clark, *op. cit.*, p. 51 et seq.

things, and all useful arts, Manufactures, Mechanic practices, Engynes and Inventions by experiment. . . .'¹ Nothing could be more explicit. Its first historian stressed this need to focus the work of scientists upon technology; in the words of Thomas Sprat in 1667 its work was intended 'for the use of cities and not for the retirements of Schools'.² Pepys urged its members to 'principally aim at such experiments or observations as might prove of great and immediate use', and had the record searched for helps to navigation. The King, petitioned by 'projectors' with secret weapons to save an industry or confound the French, referred such proposals to the Society for vetting and report. Members divided themselves into special committees for this purpose. The *Philosophical Transactions* in the seventeenth century exemplify the common concern; experiments and reports intended to have practical applications, to agriculture as well as industry, had as much space or more devoted to them as any other. This, surely, is the breeding ground for innovation. The spark then jumps from the metropolitan scene of the Royal Society in its early days to the many provincial societies linking amateur scientists with gentlemen-manufacturers in the Lunar Society of Birmingham and very many others of lesser renown. Relatively obscure towns like Spalding, Northampton, Peterborough, and Maidstone for example, boasted such gatherings. Almost thirty are known to have existed.³

William Shipley called the Northampton Philosophical Society specifically a 'Royal Society in Miniature' – 'a Society of Gentlemen that are much addicted to all manner of natural knowledge'.⁴ Most of these local societies had the specific aim

¹ See also M. Ornstein, *The Role of Scientific Societies in the Seventeenth Century* (Chicago, 1928), pp. 108–9. For details of the utilitarian aims of the French *Académie des Sciences* see chap. 5.

² T. Sprat, *History of the Royal Society* (London, 1667). See also J. G. Crowther, *The Social Relations of Science* (1967 ed.), pp. 274–87; C. R. Weld, *A History of the Royal Society . . .* (London, 1848), vol. I. pp. 146 et seq; vol. IV, section V; M. Purver, *The Royal Society: Concept and Creation* (London, 1967).

³ D. McKie, 'Scientific Societies to the end of the Eighteenth Century' in A. Ferguson (ed.), *Natural Philosophy Through the Eighteenth Century* (1948); E. Robinson, 'The Derby Philosophical Society', *Annals of Science*, vol. IX (1953); R. E. Schofield, *The Lunar Society* (Oxford, 1963); D. Hudson and K. W. Luckhurst, *The Royal Society of Arts* (London, 1954).

⁴ D. G. C. Allan, *William Shipley* (London, 1968), p. 169.

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of popularizing science and using scientific knowledge for practical ends in the improvement of practical skills in industry and agriculture – as with the national institutions of the Society of Arts (1754), founded by William Shipley, and the Royal Institution (1799), founded by Count Rumford, both of whom were passionate advocates of the application of science.

Next look at a growing list of examples of innovations which sprang, or appeared to spring, from this fertile soil of scientific discourse and social nexus between the men of science and industry. Steam-power above all; but also the adolescent chemical industry with chlorine-bleaching, sulphuric acid production, soda making, coal distillation.¹ James Watt, Dr John Roebuck, Josiah Wedgwood, Lord Dundonald, George and Charles Macintosh are the most well-known individuals who personify these connexions. The *extent* of interest in ‘amateur science’ coupled with the extent of endeavour in relating science to industry is remarkable, and in this, England is certainly unique in Europe. The important research of A. E. Musson and E. Robinson has placed all economic historians in their debt by revealing how extensive these interests were – almost, one might say, a ‘sub-culture’ of interest in science, faith in the possibilities of applying science and enthusiastic advocacy.

In fact, mathematics may well have played a wider role in these relationships than science until the end of the eighteenth century. Navigation techniques and improvements at sea (not alone sponsored by the navy), land-surveying techniques for estates, accountancy for business assaying, architectural drawing, spectacle-making are examples of practical skills that gained and were seen to gain, from mathematical knowledge. The Nonconformist and Quaker groups gave a prominent place to modern studies, particularly mathematics, that had a greater presence in new educational movements than science.²

¹ A long bibliography is contained in A. E. Musson and E. Robinson, *Science and Technology in the Industrial Revolution* (Manchester, 1969).

² S. Pollard, *The Genesis of Modern Management* (London, 1965), chap. 4; J. D. Bernal, *Science in History* (London, 1954), p. 346; N. Hans, *New Trends in Education in the Eighteenth Century* (London, 1951). A Mathematical Society was established in Spitalfields in 1717, and another at Manchester in 1718 (T. Kelly, *G. Birkebeck – Pioneer of Adult Education*, Liverpool, 1957, p. 66).

The observations of a distinguished user of the new mathematical knowledge for practical purposes underline this truth: 'We are sure of finding a Ship's place at Sea to a Degree and a half and generally to less than half a Degree,' wrote Captain James Cook, 'such are the improvements Navigation has received from the Astronomers of the Age by the Valuable Table they have communicated to the Publick under the direction of the Board of Longitude . . . [By] these Tables the Calculations are rendered short beyond conception and easy to the meanest capacity and can never be enough recommended to the Attention of all Sea Officers. . . . Much credit is also due to the Mathematical Instrument Makers for the improvements and accuracy with which their instruments are made for without good instruments the Tables would lose part of their use.'¹ The utility of such mathematical expertise coupled with precision measurement by new instruments for a trading, industrial, seafaring nation was sufficient for it to become institutionalized in schools on a fairly wide scale in eighteenth-century England. Rather than enlarge the catalogue of instances, however, let us now move to looking at some of the problems – acknowledging that a long list of such individual instances exists. It is the nature of the connexions between science and technical change, no less than the extent of the association between them which is in question.

III

The first complication is, perhaps, not a fundamental one within the European scene, although it raises important questions when relating science to innovation within a single country; or perhaps even more fundamentally when one compares scientific knowledge and its relation to technique (the general level of diffused technique rather than individual instances of 'best practice' technology) in Europe and beyond – say in China. The point is, simply, that we are much concerned with differences between national performances in industrial growth and innovation, in striving to explain the fact that the

¹ *Journals of Captain James Cook* (1768–79), ed. A. Grenfell Price (New York, Heritage Press, n.d.), pp. 112–13 (for 14 January 1773).

British economy advanced more extensively than others in this way, and became *relatively* so much more forward in adopting new techniques and developing new industries in 1750–1850 than other economies. This is particularly true of the general level of technique, productivity, and output characterizing growth industries (textiles, metal production, metal-using techniques, machine tools, machine making, particularly power engineering, chemicals, pottery, glass).

Scientific knowledge does not show, at all, the same concentration within Britain, particularly in the case of chemistry where the linkage between scientific knowledge and industrial innovation was probably most intimate. The advance of scientific knowledge was a European phenomenon. There was, in France, much greater state patronage for science through the *Académie des Sciences*, by military sponsorship, and direct industrial sponsorship, as with the research department attached to the Sèvres porcelain factory working on glazes, enamels, and paints. Provincial academies also flourished in the main regional cities.¹ In the *Description des Arts et Métiers* of Réamur 1761 one had a more elaborate schema published than any known in Britain. On balance, more systematic work was carried out in technology by scientists in France than on this side of the Channel. Countries innocent of industrialization (but with pressing military needs) also established equivalent academies, with state patronage for the useful arts – especially arts useful for military success – and much private interest in Sweden, Russia, Prussia, and Italy for example.² A Royal Irish Academy also flourished. The ‘Dublin Society for Improving Husbandry, Manufactures and other Useful Arts’ was the first of the ‘popularizing’ associations, established in 1731. The Welsh Society of Cymmrodorian followed in 1751. One of the earlier agricultural improvement societies was that of Brecknockshire, founded in 1753.³ Evidence of motivation the

¹ F. A. Yates, *The French Academies of the Sixteenth Century* (London, 1947); H. Brown, *Scientific Organizations in Seventeenth-Century France* (New York, 1934).

² R. Hahn, ‘The application of Science to Society: the Societies of Arts’, *Studies on Voltaire and the Eighteenth Century*, vols. XXIV–XXVII (Geneva, 1963), pp. 829–36. This article lists a dozen such societies in different countries.

³ H. F. Berry, *A History of the Royal Dublin Society* (London, 1915); D. G. C. Allan, *op. cit.*, p. 61.

institutionalizing of practical science in these societies clearly was; but it may well have come, in such instances, where the need was greatest, rather than where the links were closest. It should also be said that the English societies flourished with very tiny material resources indeed, being amateur and self-financing. The very small cash premiums or medals they offered as inducements to inventors cannot be seen as 'research and development' costs in the modern sense of capital investment in innovation. The fact that endeavour was stimulated by the chance of winning a medal offered by such a private society or appearing in its transactions, says much for the prestige attached to science and to the quest for 'improvement' in practical matters. But clearly these investments and endeavours could be made on a scale more extensive absolutely than England (in the case of France) and on a scale relatively greater (judged against the resources of the country) without much of a 'fall out' giving a boost to industrial growth.

The French record of scientific growth and invention in the eighteenth century was a formidable one.¹ Berthollet first revealed to the world the bleaching possibilities of chlorine, first isolated as a gas in 1774 by a Swedish chemist Scheele, which was followed by energetic efforts to promote its manufacture in France. A similar sequence followed with Leblanc making soda from salt and sulphuric acid.² Very sophisticated work was done in the production of dyestuffs in France; with varnishes, enamels, and many other techniques and materials. Yet the difference in the rate of industrial growth based on these advances in chemistry between France and Britain in the period 1780 to 1850 was remarkable. Almost all the theoretical work on structures, stresses, and the mechanics of design in civil engineering was French. This did not appear to have much relationship to the speed of development, or even innovations in these fields, as far as economic progress was concerned. The same was true of power engineering and hydrodynamics.³

¹ See, in illustration, S. T. McCloy, *French Inventions of the Eighteenth Century* (Lexington, Ky., 1952) and *Government Assistance in Eighteenth-Century France* (Durham, N.C., 1946).

² C. C. Gillespie, 'The Discovery of the Leblanc Process', *Isis*, vol. XLVIII (1957).

³ D. S. L. Cardwell, 'Power Technologies and the Advancement of Science,

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The record of development and implementation was also significantly different from the record of invention.

The wider question, not to be pursued here, is even more interesting. The sophisticated scientific mechanical knowledge in China produced even less impetus to the general levels of industrial technique representative of that vast region or to industrial growth. It remained more sealed up in a small enclave of scholars, civil servants, and isolated groups under noble and royal patronage than in – say – St Petersburg. By itself, therefore, it becomes difficult to argue that a flow of new scientific knowledge and applied science was a key variable; it may be a pre-condition for advance, but it does not necessarily give the operational impetus.

Secondly, the problem of time-lags between knowledge and action raises awkward problems for the ‘positive’ one-to-one equation in its simple form. The economic historian is more interested in innovation and the diffusion of innovations than in invention for its own sake. Putting inventions to productive use involves all the costs and problems of translation from laboratory technique into industrial production, from the largely non-commercial context of the pursuit of knowledge to profitability as a condition of existence. One is not even very interested in isolated examples of new industrial techniques but rather in their diffusion to the point when innovations begin to affect general levels of output, costs, productivity in an industry; when their adoption is on a sufficient scale to affect the performance of the industry significantly. To mention a few of these astonishing time-lags. The screw-cutting lathe, foundation of the precision engineering skills which made an efficient machine-making industry possible, was clearly documented by Leonardo da Vinci in the *Note-Books*, laid out again in the section on watch-making tools in the *Description des Arts et Métiers* in mid-eighteenth century France and developed, spontaneously, again by Maudslay, to become – from that

1700–1825’, *Technology and Culture*, vol. VI (1965); ‘Some Factors in the Early Development of the Concept of Power, Work and Energy’, *British Journal for the History of Science*, vol. III (1966–7); R. Hahn, *L’hydrodynamique au XVIII^e siècle* (Paris, 1965); D. Landes, *Cambridge Economic History of Europe*, vol. VI (Cambridge, 1965), p. 333.

innovation – the basis of a progeny of machine tools. Sir T. Lombe's silk-throwing machine, which was used for the first time in a factory in England in 1709, had been used and known in Italy since 1607 – with an accurate engraving in a book on the open shelves of the Bodleian Library by 1620.¹ The same is true of gearing and the design of gear wheels, bridge design, pumps, Archimedian screws, the 'pound' lock, mass production needle-grinding machines and a host of others (all to be found in Leonardo's work).² In certain respects, steam power is another example. The pound lock – being the basic technology of a dry dock – was known in Dutch shipyards in the fifteenth century, perhaps much earlier, and appeared in England in the sixteenth century. But a still-water canal system of which this is the only important piece of technology was an eighteenth-century phenomenon in Britain.³ Equally dramatic time-lags exist in the opposite direction – between empirical improvements in technique and the beginning of scientific interest in explaining them.⁴

Bound up with this problem of time-lags between knowledge and invention, invention and adoption, adoption and diffusion are correlated phenomena such as simultaneous inventions (developed spontaneously and independently in different places at about the same time), re-inventions of lost techniques, 'alternative' inventions coming very close together in time for providing different ways of getting the same thing done.⁵

The 'profile' of technical change usually shows an evolutionary curve as well as revolutionary discontinuities. The interstices between the discrete advances made by identifiable

¹ Quoted G. N. Clark, *op. cit.*

² I. B. Hart, *The World of Leonardo da Vinci* (London, 1961).

³ Surveying techniques were certainly advanced enough in the late sixteenth century to facilitate canal cutting. The New River project bringing water from Hertfordshire to North London involved very sophisticated routing and exactness in calculating levels.

⁴ A. R. Hall and T. S. Kuhn in M. Clagett, *Critical Problems in the History of Science* (Madison, Wisc., 1959), pp. 16–17.

⁵ R. K. Merton, 'Singletons and Multiples in Scientific Discovery', *Proceedings of the American Philosophical Society*, vol. CV (1961); W. R. Maclaurin, 'The Sequence from Invention to Innovation . . .', *Quarterly Journal of Economics*, vol. LXVII (1953).

individuals are filled by 'continuum' improvements made on the job by countless improvements without known, or identifiable and published authorship. Collectively the latter may yield a cumulative advance in productivity greater than the identifiable discrete innovations. This has been likened to biological change; improvement and survival by the techniques most efficiently and economically adapted to their function – a kind of technological Darwinism.¹ The burden of all this is, of course, that invention waits upon economic opportunity before it can come to fruition in innovation and the diffusion of new techniques. The determinants of timing are usually set – in the long run – by non-technical criteria. These determinants may be economic criteria of different sorts – the widening of the market giving inducement for larger production and hence new methods, greater facility in the supply of capital, a change in factor prices² (for example, labour becoming relatively more expensive or intractable, raising the incentives to cut labour costs). Boom conditions, creating bottle-necks in supply, higher profits, and greater incentives to expand may create the operational incentives. In a dynamic sequence, when an economy is on the move with innovations flowing, a depression may equally induce further innovation by creating pressures to cut costs. The process of innovation itself creates a disequilibrium in various ways – that dis-equilibrium, to be resolved, creating the need for further changes, which become self-reinforcing. These may indeed be technical in nature, but they are need-creating in the way they operate. The causal arrows flow from industrial demand towards the absorption of new knowledge. The timing is set from within the industrial rhythm and the economic context rather than given to it exogenously by new acquisitions of knowledge.³ There are

¹ S. C. Gilfillan, *The Sociology of Invention* (Chicago, 1935); 'Invention as a Factor in Economic History', *Journal of Economic History*, Supplement (1945).

² '... every price change, by creating cost difficulties in certain fields and opportunities for profit-making in others, provides a double stimulus to invention' (A. Plant in *Economica*, 1934, p. 38, quoted Clark, op. cit.).

³ J. Schmookler, *Invention and Economic Growth* (Cambridge, Mass., 1966); 'Economic Sources of Inventive Activity', *Journal of Economic History*, vol. XXII (1962); W. F. Ogburn and D. Thomas, 'Are Inventions Inevitable?', *Political Science Quarterly*, vol. XXXVII (1922); R. C. Epstein, 'Industrial Inventions: Heroic or Systematic?', *Quarterly Journal of Economics*, vol. XL

other determinants – social, political, and legal affecting the conditions of risk. ‘Entrepreneurship’ may also prove to be greater than the sum of these other criteria.

These sorts of motivations tend to be the operational criteria in this period, I believe, determining which bits of scientific knowledge were taken up, developed, applied, and which lay unused; which inventions remained known, but sterile, and which quickly became adopted, perhaps outside the country which gave them birth. Clearly, this is the style of explanation behind the very rapid adoption of ‘chemical’ bleaching in the cotton industry in Britain more than in France: the enormous expansion in the output of cloth made a more rapid means of bleaching imperative. Great stress has recently been placed by historians of science on the ways in which empirical processes and skilled artisan technology in the mechanical arts stimulated scientific advance in these centuries.¹

However, this does not necessarily subvert the core of the ‘positive’ equation. It can be argued that applied science needs to build up a capital ‘stock’ or a ‘library’ of knowledge, so to speak, which is thereupon available for industrialists to draw upon in innovation – across national frontiers, no doubt, for science now enjoyed, in printing, a very effective means of diffusing knowledge in the seventeenth century and after; and

(1926); R. K. Merton, ‘Fluctuations in the Rate of Industrial Invention’, *Quarterly Journal of Economics*, vol. XLIX (1934-5).

The very large literature about technical change and innovation now developing is seeking to establish criteria for measuring and evaluating this phenomenon in economic theory. Almost all of it relates to twentieth-century examples and assumptions, particularly that concerned with research costs and applied science. Conclusions are therefore not *ipso facto* applicable to innovation as a phenomenon in the seventeenth and eighteenth centuries.

¹ E. G. P. Rossi, *Francis Bacon* (London, 1968), p. 9; E. J. Dijksterhuis, *The Mechanization of the World Picture* (Oxford, 1961), pp. 243-4; T. S. Kuhn, ‘Energy Conservation . . .’ in M. Clagett, op. cit.; A. R. Hall, *From Galileo to Newton, 1630-1720* (London, 1963), pp. 329-43; A. R. Hall, *The Scientific Revolution*, 2nd ed. (London, 1962), pp. 221, 225, 236; J. D. Bernal, *Science in History* (London, 1954), pp. 345-6, 371. At its extreme, this case becomes the doctrinaire Marxist position that advances in scientific knowledge were determined purely by the bourgeoisie’s commercial and industrial needs. See B. Hessen, *Science at the Cross-Roads* and the debate given in condensed form in G. Basalla (ed.), *The Rise of Modern Science* (Boston, D. C. Heath, 1968).

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with a timing doubtless profoundly influenced by conditions and incentives within industry. But, without that capital stock produced by the advance of scientific knowledge, runs the argument, a limit would have been placed upon the range of advance.

This begs the question as to how far the impetus deriving from industry was able to produce the conditions – of innovation as well as other things – needed to sustain its own progress. To what extent was the flow of innovations produced from within the empirical world of industry and not given to it from an ‘exogenous’ world of science, advancing under its own complex of stimuli? That in turn begs the question as to how far industrial demand, with the needs of the ‘empirical’ world, was itself the stimulus for creating new scientific knowledge in this period, of what ‘feeds-back’ or ‘feeds-forward’ there were?

Judging the effectiveness of the contributions of science by results, *ex post facto*, rather than by endeavour, is to greatly reduce their importance. Little of the mass of experimentation in agricultural projects of the Royal Society in its early years, for example, seems to have had much direct effect upon improving the efficiency of farming.¹ Most of the more direct links between advancing knowledge of chemistry and the expanding chemical industry came only at the end of the eighteenth century. This disconnects the timing of much of the new knowledge, particularly in chemistry, from the initial phases of industrial growth. The great advances in mechanics in the seventeenth century – then one of the most advanced of the sciences – had given birth to very sophisticated theoretical schema about ballistics, which do not appear to have significantly affected the processes of innovation in making metals or working metals, of gunfounding or of gunnery, again judging by result, until after the Crimean War. The ‘science’ remained almost purely abstract. Each cannon cast and bored remained slightly different from each other; each shot and each charge of powder were equally ‘unique’, which kept techniques of gunnery strictly empirical. A precision engineering industry to produce the guns, and a precision

¹ See below, pp. 89–91.

chemical industry to produce the propellants, were required before this theoretical knowledge could become operational.¹

The empirical stimulus creating response within the immediate context of production accounts for a very high proportion of the advance in productivity, even in those industries most exposed to the impact of science, and a great determinant of timing and the rate of diffusion of new techniques. Great areas of advance were relatively untouched by scientific knowledge, judging by result rather than by intention or endeavour, until the nineteenth century: agriculture, canals, machine-making, the mechanization of cloth-making (as distinct from bleaching and dyeing), iron- and steel-making. Taking the occupational census of 1851, a very small percentage indeed of the labour-force was engaged in trades where the linkages were – superficially at any rate – high, as in chemicals.

IV

Steam power is important enough to merit separate attention. Here was the greatest gift from science to industry, it has often been claimed, born exactly from the world of the Royal Society, of noblemen's laboratories, from an international competition among scientists and their leisured patrons in the seventeenth century. Watt, in his generation, carried on this precise linkage between scientific knowledge and commercial application in the series of formal experiments to analyse the properties of steam and the conductivity of metals which lay behind his own inventions of the separate condenser and steam power proper (as distinct from 'atmospheric' power). This can be called the classical example of science in alliance with practice.² But other factors also conditioned the rates of advance of efficiency in steam power and the timing of stages of growth of this innovation. Thomas Newcomen and Savery were not so directly within this educated scientific tradition. And historians of science continue to push back the genealogy of the basic scientific awareness of steam power – knowledge

¹ A. R. Hall, *Ballistics in the Seventeenth Century* (Cambridge, 1952).

² G. N. Clark, *op. cit.*, p. 21.

as distinct from laboratory experiments that worked.¹ The jump to the world of Thomas Newcomen, who had no personal contacts with the leading scientists of the day,² fashioning an effective commercial device, meant problems of manufacture, of standards of accuracy in metalworking, that alone made effective use possible on a commercial basis. This, it can be argued, more than anything else set the limits of efficiency. And this, as a blacksmith, was Newcomen's world, not that of the Royal Society. Once again, the context within which Watt's inventions had to become operational meant that the accuracy of working metal, of fitting a piston to a cylinder throughout its length, of getting steamproof valves and joints set the limits to the rise in the degree of efficiency which potentially resulted from the new inventions. These efficiencies came from the empirical world of John Wilkinson and Matthew Boulton, with rising standards of its own, a world increasingly working to rule, but still mainly innocent of formal scientific thought.

And, subsequent to Watt, most of the pioneering of 'high-pressure steam', the adaptation of steam to traction, to small-bench engines, to ships and the continuum of improvement to the Watt-style engine itself, belonged, for the most part, to the empirical world of the obscure colliery engineers, the captains of Cornish mines, the brilliant mechanics such as Murdock. Some of them were trained in the best precision workshops of the country, such as Maudslay's, but remained nevertheless innocent of scientific fundamentals and were not seeking to create their improvements in the light of awareness of such fundamentals. Yet the cumulative total of 'continuum' innovation, effected on the job, at the work bench, bit by bit, were profound. Taking steam-power efficiency again as one example, the first Newcomen engine had a duty³ of *c.* 4.5 m, it has been

¹ A. R. Hall has gone so far as to state: 'No scientific revolution was needed to bring the steam engine into existence. What Newcomen did could have been done by Hero of Alexandria seventeen hundred years before, who understood all the essential principles' (*From Galileo to Newton, 1630-1720*, London, 1963, p. 333).

² D. S. L. Cardwell, *Steampower in the Eighteenth Century* (London, 1963) p. 18.

³ The number of pounds of water raised 1 foot by the consumption of 1 bushel of coal.

calculated.¹ This had been raised to 12.5 m by the time of Smeaton's improvements in 1770. The initial Watt separate condenser engine raised this duty to c. 22 m. By 1792 it had been raised by continuing improvement to over 30 m. The best recorded Watt-type engine in 1811, working in Cornwall, had a duty of 22.3 m. In 1842-3, under continuous gradual improvement, the best duty was recorded at 100 m. Average duty rates recorded on Cornish engines quadrupled between 1811 and 1850 as a result of this continuum-type improvement.

V

Although this paper is primarily concerned with sources of innovation in industrial techniques, improvements in agriculture are relevant to the issue, both because agriculture potentially stood to gain from the application of science, comparably to industry (and has gained as dramatically from its connexion with science as has industry in the twentieth century), and because contemporaries certainly gave agricultural improvement at least as high a priority as industrial advance. Agricultural improvement also had a more general appeal to the upper and middle classes of English society than any other branch of production, if only because larger and more influential social groups were concerned with the land. Agricultural innovations and scientific experiments in agriculture featured in virtually all the scientific and philosophical societies mentioned above; while other societies were specifically agricultural in their terms of reference. The Georgical Committee of the Royal Society was established in 1665 (as one of eight sectoral groups). Many experiments in husbandry found a place in the *Philosophical Transactions* over the years; and eleven reports are known to have been made about agricultural practices, produced from national inquiries.² A 'Society of Improvers in the Knowledge of Agriculture in Scotland' was founded in 1723 (forty years before that in

¹ D. B. Barton, *The Cornish Beam Engine* (Truro, 1966), pp. 28, 32, 58. These figures are suspect, but there is no reason to suppose that the degree of suspicion advanced with time – rather the reverse. Other factors went into these duty counts as well as the intrinsic technical potentiality of the engines.

² R. L. Lennard, 'Agriculture under Charles II', *Economic History Review* (1932).

Brecknockshire) and many local agricultural societies followed, particularly in the 1790s.¹

Scientists concerned themselves with agricultural experiments, advocating the experimental method and lauding the claims of science in farming no less than in industry. Francis Bacon's *Sylva Sylvarum* (1651) included a comparative study of different modes of fertilizing. In 1671, Boyle urged farmers to experiment: 'Chymical experiments . . .' he wrote, 'may probably afford useful direction to the Husbandman towards the melioration of his land, both for Corn, Trees, Grass and consequently Cattell.'² John Evelyn's various works contained a curious – if typical – mixture of magic and shrewd common-sense, in circumstances where virtually anything organic, and much inorganic, could be thrown on to the land with advantage.³ Hale's *Vegetable Statics* (1727) continued the line, for the first time challenging the view that plants were composed simply of water. Francis Home, both a doctor and subsequently a professor in Edinburgh, also deliberately set out to apply science to agriculture to see 'how far Chymistry could go in settling the Principles of Agriculture'.⁴ Lavoisier set up agricultural experiments and ran a model farm.⁵

Thus, by intention and endeavour, agricultural innovation shares with industry a common link with science in the seventeenth and eighteenth centuries. The problems about concluding from these aspirations and associations that applied science was the prime source of such innovation in agriculture as actually occurred are even greater, if anything, than with industry. There, at least, in the specialized sector of the chemical industry and immediately related fields there is solid evidence for the connexion in a direct way (though causal links could flow in both directions). Much theorizing was

¹ These include the Canterbury Agricultural Society, Odiham Society, London Veterinary College, Bath and West and Southern Counties, Norfolk Agricultural Society.

² R. Boyle, *Some Considerations Touching the Usefulness of . . . Natural Philosophy* (Oxford, 1671).

³ J. Evelyn, *Sylva* (London, 1664); *Kalendarium Hortense* (London, 1664); *A Philosophical discourse of earth . . .* (London, 1676).

⁴ F. Home, *Principles of Agriculture and Vegetation* (Edinburgh, 1757).

⁵ E. J. Russell, *A History of Agricultural Science in Great Britain, 1620–1954* (London, 1966), p. 53.

plainly mistaken, based on quite irrational premisses. The innovations which characterized progressive farming did not owe much, if anything, to such science; while only the very exceptional farmer or landowner was directly influenced by the scientists. Russell, as a leading present-day agricultural scientist, is profoundly sceptical of the relevance of chemistry to agricultural advance before the generation of Leibig, Davy's *Elements of Agricultural Chemistry* (1813), and the Rothamsted experiments of Lawes and Gilbert in the 1840s.¹

But scepticism about the significance of the direct application of formal scientific knowledge to agrarian improvement in these two centuries does not end the story of the relevance of this evidence. To anticipate a tentative conclusion drawn below, one can certainly take this large body of data as strong evidence of *motivation* for agrarian advance. Coupled with false premisses about chemical reactions were urgent pleas for experimentation, shrewd observation and recording, the comparative method, seeking alternative ways of doing things which could be measured and tested to see if they were superior to the old. This was a programme for rejecting traditional methods justifiable only because things had always been done in that way (even though such customs, hallowed by the passing of time, often did embody strict rationality, even if unself-conscious and inarticulate on the lips of their practitioners). Scientific procedures and attitudes encouraged by the scientists may have been more influential than the scientific knowledge they dispensed. 'I should not, therefore, proceed a single step', wrote Francis Home, 'without facts and experiments.'²

The publicity given to new methods, new crops, rotations, and implements by these same groups may also have increased the pace of diffusion of innovations in agriculture. Certainly the flood of writing, at all different levels, is evidence of an intellectual world where progress was written into the assumptions of the age.

¹ E. J. Russell, op. cit., pp. 25, 37, 46. See also G. E. Fussell, 'Science and Practice in Eighteenth-Century British Agriculture', *Agricultural History*, vol. XLIII (1969). But see the comments of D. J. Brandenburg.

² F. Home, *Principles of Agriculture and Vegetation* (Edinburgh, 1757). He urged the Edinburgh Society to raise 'a spirit of experimental farming over the country'.

VI

The institutional development of science creates certain problems, because the patterns of development in science do not always fit sequences of innovation and development in industry. The foundation of the Royal Society and the Lunar Society are always quoted in evidence of the developing nexus between science and industry. But one must then face the issue of the decline in the utilitarian orientation towards applied science of the Royal Society after 1670, its decline during the first half of the eighteenth century and in parts of the nineteenth century, and its great weaknesses compared with the equivalent academy in France. The Lunar Society also withered into a state of collapse after the 1790s. In some fields, also, it seems perfectly possible – even in that most applied of sciences, medicine – for the accumulated advances of several generations, well institutionalized, not to result in any major impact upon national demographic trends in death-rates or disease-rates. Medicine, we are told, did not begin to have such a major impact – that is, upon a statistically significant proportion of the population – until the second half of the nineteenth century, apart, perhaps, from the effects of inoculation and vaccination on smallpox. And this was in a field where there was very great interest, considerable advances in scientific knowledge, a greater flow of money, and probably more scientifically trained persons than in all other branches of science put together.¹

The problem of numbers is also relevant. Persons professionally trained in medicine, in chemistry, the ‘scientific’ members of the Royal Society (as distinct from the much larger number of ‘gentlemen’ innocent of professional commitment) remained the merest handful. The average number of elections to the Royal Society in the early eighteenth century was about ten. The average number elected in each year to the College of Physicians was five or six before 1700, supplemented only marginally with those with degrees from foreign universities. These numbers did rise after 1700 but remained

¹ This conclusion may need to be modified in the light of research being currently undertaken by Dr E. Sigsworth of York.

very tiny. This may be taken as an index of the professionalization of science to the extent that this term possessed its modern connotation in the eighteenth century. Beyond this there were, of course, much larger numbers of amateurs and people in business, such as opticians and distillers, who practised empirical science in a commercial way. As Professor Hall has said: '... the impact of any question of abstract science upon a human brain was exceptionally infrequent – it could only happen to, say, one individual in a hundred thousand. But, in the history of technology the situation is very different; the proportion of human beings who could have been very well acquainted with handmills and ploughs and textile- and horsegear has always been very large indeed, until the last century or so in the West. Very few of these ever effected the slightest variation in any technique; but the potentiality for effecting a variation was virtually universal. It is only when the use of relatively uncommon machines or techniques is introduced that the potentiality for innovation becomes restricted.'¹

In England no great expansion of institutions in 'professional' science developed in this period outside the Royal Society, either within the universities or outside them, apart from the amateur groups and the popularizers. Science did not become an established part of the educational system, either within the traditional institutional hierarchy or beyond it in special organizations of its own. The Mechanics' Institutes subsequently became the only widespread response to be institutionalized in this way in the first half of the nineteenth century, and they trained the aspiring literate artisan, not the research chemist. The greatest contrast existed between the English experience and French and German developments in the *École Polytechnique* and the *Technische Hochschule*. The School of Mines and the Royal College of Chemistry in London were the two institutions that challenged this generalization. They remained very small, isolated geographically, socially, and technically from having any significant impact upon mining or industry in Britain as a whole during the greater part of the nineteenth century.

¹ A. R. Hall, *The Historical Relations of Science and Technology* (London, 1963).

VII

In conclusion: it was the same western European society which saw both great advances in science and in technological change in the great sweep of time and region across the fifteenth to the nineteenth centuries. It would be carrying nihilism to the point of dogma to write this off as a mere accident, even though the case of China suggests that it is perfectly possible for sophisticated scientific and technological knowledge in some fields to produce a very small impetus towards lifting general levels of industrial technique. The simplest assumptions of causation flowing directly and in one direction need to be questioned; the presumption that connexions between science and industry were direct, unitary, simple. Negatively it can be argued that the many other conditioning factors in technical change were collectively of much greater importance during the first century of industrialization and that, in the immediate context of manufacture, formal scientific knowledge was much less strategic in determining commercial success than some modern studies have suggested. In longer perspective we may see that the main impetus from formal applied science to innovation came after 1850 on an over-widening front, but in a context which was highly favourable for many other reasons. That this was the real pivot in the connexions between science and industry was shown by default, to a large extent, in the case of Britain lagging most in exactly those fields of innovation where the connexion was becoming most intimate.

But much depends upon whether we are looking at the immediate context of innovation or at the general nature of the society, and its intellectual parameters, within which industrial advances were burgeoning. 'We have to see,' as G. N. Clark concluded long ago, 'not a gradual and general mutual approach of these elements in society, but the joining of contact, first at isolated points, then at more points; finally almost everywhere.'¹ Until the end of the eighteenth century – that is, until long after systematic, cumulative change on a scale quite uncharacteristic of medieval technical change was under

¹ G. N. Clark, *op. cit.*, p. 22.

way – that inter-penetration was confined to fairly small areas, even if some of them were strategic.

It should also be acknowledged that scientific attitudes were much more widespread and diffused than scientific knowledge. Attitudes of challenging traditional intellectual authority, declining lines of development by observation, testing, experimentation, and adopting – indeed, actively stimulating the development of – scientific devices such as the thermometer and hydrometer, which enabled industrialists to reduce their empirical practices to rule wherever possible, were certainly being strengthened.¹ The quest for more exact measurement and research for the means to fulfil it was certainly characteristic of these linkages, even where the object was not to subvert empirical techniques, of which the chemistry remained unknown, but to standardize best practice within them. Scientific devices and techniques were thus often used to buttress empirical techniques rather than to challenge them. In this sense, the developing Baconian tradition of the experimental sciences, the tradition of research based upon systematic experimentation (as in late eighteenth-century chemistry) had closer links with the process of innovation than did advances in cosmology, mechanics, or physics in the seventeenth century. And in such linkages science probably learned as much from technology as technology from science until the nineteenth century: scientists were much concerned with trying to answer questions suggested from industrial techniques. ‘Technological progress implied the idea of intellectual progress, just as chance discoveries implied the possibility of systematic ones.’²

We may conclude that together both science and technology give evidence of a society increasingly curious, increasingly questing, increasingly on the move, on the make, having a go, increasingly seeking to experiment, wanting to improve. This may be the prime significance of the new popularizers of science and technology, the encyclopedias, the institutions like the

¹ The brewing and distilling industries, with the excise authorities anxious to have more precise calculations in gauging for taxation, offer a good example of such a sequence. See P. Mathias, *The Brewing Industry in England, 1700–1830* (Cambridge, 1959), pp. 63–78.

² A. R. Hall, *The Scientific Revolution* (London, 1962), p. 369.

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Society of Arts, the Royal Institution, the Lunar Society and the various local philosophical and scientific societies, the new educational movements, the intriguing links between radical nonconformist scientific and business groups in the eighteenth century or between Puritans and the founders of the Royal Society in the seventeenth century. So much of the significance, that is to say, impinges at a more diffused level, affecting motivations, values, assumptions, the mode of approach to problem-solving, the intellectual milieu, rather than a direct transference of knowledge. In this sense, of course, the conclusion is banal, that the advances in science and in technical change should both be seen as characteristics of that society, not one being simply consequential upon the other.

2 *The Diffusion of Technology in Great Britain during the Industrial Revolution*

A. E. MUSSON

The following extracts are taken from chapter II of A. E. Musson and E. Robinson, *Science and Technology, in the Industrial Revolution*, Manchester University Press, 1969.]

Was it [the Industrial Revolution], as Professor Landes has said, achieved through the efforts of 'practical tinkerers',¹ or did it have some scientific basis? There is no doubt that native empiricism was of immense and probably predominant importance. It was strongly evident, for instance, in the development of engineering and industrial motive power – the bases of modern mechanized mass-production. Mr Robinson and I have investigated the origins of engineering in Lancashire, where the early factory system had its most striking developments.² We have traced how early engineering workers were recruited from a wide range of traditional skilled craftsmen; smiths, wheelwrights, millwrights, carpenters, turners, clockmakers, etc., in fact from all kinds of workers in metal and wood. The millwright – the jack-of-all-trades described by Fairbairn³ – was especially important. So, too, as we have demonstrated, was the clock-maker, whose skills and tools

¹ D. S. Landes, 'Entrepreneurship in Advanced Industrial Countries: The Anglo-German Rivalry', *Entrepreneurship and Economic Growth*, papers presented at a conference in Cambridge, Massachusetts (November 1954), p. 8. He has somewhat revised this view, however, in the *Cambridge Economic History of Europe*, vol. VI (1965), part I, pp. 293–6, in the light of the evidence we have produced.

² A. E. Musson and E. Robinson, 'The Origins of Engineering in Lancashire', *Journal of Economic History* (June 1959); also chap. XIII in *Science and Technology*.

³ W. Fairbairn, *Treatise on Mills and Millwork* (2 vols., 1861–3), vol. I, pp. v–vi; Musson and Robinson, *Science and Technology*, pp. 73, 429, 481.

were turned to cutting gear-wheels, etc. for the 'clockwork' of early textile machinery. A similar transition is uniquely illustrated in Pyne's *Microcosm* of 1808,¹ where we see how the making of wooden cart wheels metamorphosed into the construction of wooden 'wheel machinery' (spur wheels, crown wheels, pinions, etc.) for the early mills. Brindley was such a wheelwright and millwright. Similarly, craftsmen from other trades – notably Smeaton (instrument-maker) and Rennie (millwright) – developed the use of cast-iron gears and other mechanical improvements; Rennie, for example, appears to have brought the centrifugal governor for steam engines – the invention of which is often attributed to James Watt – from his millwright practice in windmills.² In the same way, turners in wood changed easily into turners in metal. . . . Iron-founders, of course, were especially important in this development of engineering. . . .

Traditional empirical skills were undoubtedly important. . . . It should also be emphasized that traditional forms of power remained far more important in the industrial revolution than is generally realized. Water power was especially important. We know, of course, that water-wheels had long been used for grinding corn and fulling cloth, and increasingly, from the sixteenth century onwards, for working mine-drainage pumps and winding engines, for operating furnace-bellows, hammers, and rollers in iron-works, for driving saw-mills and paper mills, and for many other industrial purposes. We know, too, that early textile factories, such as Lombe's silk mill and Arkwright's cotton mills, were powered by water-wheels. But the enormous number and wide variety of uses of such wheels have never been fully appreciated, nor have the improvements in their design and their increasing size during the Industrial Revolution. . . .

These advances in water power deserve stressing because they have been unduly overshadowed by the early develop-

¹ W. H. Pyne, *Microcosm*, vol. II (1808), plate 69, reproduced in W. H. Chaloner and A. E. Musson, *Industry and Technology* (1963), plate 64.

² C. T. G. Boucher, *John Rennie 1761-1821* (1963), pp. 11, 81-4. Mr Robinson, however, considers that Watt developed the use of this device independently, though also from windmills.

ment of steam engines. In the long run, of course, steam power was to be of far greater importance, but it was a longer run than is generally realized. . . .

The early steam-engine makers [also] came from the ranks of ironfounders, millwrights, and other traditional trades, as previously mentioned. Watt himself was originally an instrument-maker, like Smeaton. They were able to apply their practical skills to the solution of new engineering problems. Probably most of the achievements of the early Industrial Revolution were similarly products of practical empiricism. But during the past decade or so an increasing amount of evidence has been accumulating to show that scientific technology was also developing at this time and assisting in these achievements. Dr and Mrs Clow, for example, have shown how the Scottish universities contributed to advancements in chemical technology in a wide range of industries, such as the alkali, soap, glass, bleaching, dyeing, and other trades.¹ Many similar examples can be culled from the pages of *Isis*, the *Annals of Science*, and the growing number of similar journals. Mr Robinson and I have also tried to show the industrial significance of philosophical societies in Birmingham, Manchester, and other towns, of dissenting academies, embryo technical colleges and schools, of books, encyclopedias, periodicals, and libraries, and of itinerant lecturers – all of which helped to spread scientific-technological knowledge and interests more widely.² Dr Schofield has more recently demonstrated in copious detail the links between science and industry in the Birmingham Lunar Society.³

Professor Jewkes and his collaborators have suggested that 'the disposition of modern writers to regard nineteenth- [and eighteenth-] century inventors as uneducated and empirical in their methods is a direct outcome of the difficulty which academically educated people often have in understanding the possibilities of self-education'.⁴ As they briefly observe,

¹ A. N. and N. Clow, *The Chemical Revolution* (1952).

² Musson and Robinson, 'Science and Industry in the Late Eighteenth Century', *Economic History Review*, vol. XIII, no. 2 (December 1960), and *Science and Technology in the Industrial Revolution*, chap. III.

³ R. E. Schofield, *The Lunar Society of Birmingham* (1963).

⁴ Jewkes, *op. cit.*, pp. 63-4.

facilities for self-education certainly existed, and some of the most notable scientists and industrialists were self-educated.¹ Many of the early engineers, for example, combined scientific knowledge with practical experience. Even the ordinary eighteenth-century millwright, according to Fairbairn, was generally 'a fair arithmetician, knew something of geometry, levelling, and mensuration, and in some cases possessed a very competent knowledge of practical mechanics. He could calculate the velocities, strength and power of machines; could draw in plan and in section . . .': indeed they were commonly regarded as men of 'superior attainments and intellectual power'.² The truth of this statement is clearly evident, not only in Fairbairn's own works, but in those of earlier engineers such as Smeaton, Telford, and Rennie. Smeaton's high abilities in surveying, designing, mechanics, etc. are revealed in the four volumes of his *Reports*, published by the Smeatonian Society of Engineers in 1812, and also in his drawings,³ while his capacity for scientific-technological experiment is brilliantly demonstrated in 'An Experimental Inquiry concerning the natural Powers of Water and Wind to turn Mills, and other Machines',⁴ and by his similar investigations into the atmospheric steam engine, all scientifically controlled, with results tabulated mathematically.⁵ Hence his election as a Fellow of the Royal Society, to which he read numerous other papers on

¹ Faraday, Davy, Sturgeon, and Wheatstone are among the 'self-educated' scientists mentioned by Jewkes; Young, Hodgkinson, and others could be added. We shall be referring to numerous industrialists who similarly acquired scientific knowledge.

² W. Fairbairn, *Treatise on Mills and Millwork* (1861-3), vol. I, pp. v-vi.

³ See *A Catalogue of the Civil and Mechanical Engineering Designs, 1741-92, of John Smeaton, FRS, preserved in the Library of the Royal Society* (Newcomen Society Extra Publication no. 5, 1950).

⁴ Smeaton made his experiments in 1752-3. The results formed the subject of papers read to the Royal Society in May and June 1759 (*Philosophical Transactions*, vol. LI, 1759, pp. 100-74), for which he was awarded the Society's Copley Medal. These papers were republished in 1794. These and his other papers on mechanics were frequently referred to by later engineers.

⁵ These experiments, made in the early 1770s, are explained and analysed in detail by J. Farey, *A Treatise on the Steam Engine* (1827), pp. 134, 158, 166 ff. The experiments, of which he recorded over 130, were spread over four years.

mechanics,¹ scientific instruments,² and astronomy. During his visits to London, 'it was a source of great pleasure to him to attend the meetings of the Royal Society, as well as to cultivate a friendship with the distinguished members of the Royal Society Club'.³ He was one of the leading figures in the earliest society of engineers, founded in 1771, which, after his death, became the Smeatonian Society in 1793 and was the forerunner of the Institution of Civil Engineers, founded in 1818.⁴ The members, who included most of the leading engineers of that day, discussed engineering theory as well as practice. One of their meetings in 1778, for example, was spent 'canallically, hydraulically, mathematically, philosophically, mechanically, naturally, and socially'.⁵

Smeaton, so Smiles tells us, deliberately 'limited his professional employment, that he might be enabled to devote a certain portion of his time to self-improvement and scientific investigation'; he was frequently engaged in study and experiment in the specially erected tower, combining workshop, study, and observatory, at his home at Austhorpe, near Leeds.⁶ He was also a member of the scientific coterie in Leeds, including Joseph Priestley and others.⁷ George Stephenson later referred to Smeaton as

the greatest philosopher in our profession that this country has yet produced. He was indeed a great man, possessing a truly Baconian mind, for he was an incessant experimenter. The principles of mechanics were never so clearly exhibited as in his writings, more especially with respect to resistance, gravity, and the power of water and wind to turn mills.

¹ Notably 'An Experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of Rest', read 25 April 1776, *Philosophical Transactions*, vol. LXVI (1776), pp. 450-75, and 'New Fundamental Experiments upon the Collision of Bodies', read 18 April 1782, *ibid.*, vol. LXXII (1782), pp. 337-54. Smeaton refuted the theoretical opinions of earlier philosophers such as Belidor, Parent, Desaguliers, Maclaurin, etc. on these matters, especially in regard to the operation of water-wheels.

² See, for example, Musson and Robinson, *Science and Technology*, p. 50, n. 4.

³ Smiles, *Lives of the Engineers* (1874), vol. II, p. 169.

⁴ S. B. Donkin, 'The Society of Civil Engineers (Smeatonians)', *Newcomen Society Transactions*, vol. XVII (1936-7).

⁵ *Ibid.*, p. 58.

⁶ Smiles, *op. cit.*, chap. VI.

⁷ See Musson and Robinson, *Science and Technology*, pp. 157-8.

His mind was as clear as crystal, and his demonstrations will be found mathematically conclusive. To this day there are no writings so valuable as his in the highest walks of scientific engineering . . .¹

Telford, originally a stonemason, is often regarded, like Brindley before him, as a purely practical engineer, devoid of, and even hostile to, scientific theory.² There is no doubt, as Sir Alexander Gibb has shown,³ that Telford emphasized above all the value of practical experience, but he was 'no enemy to either mathematics or theory'. Indeed, he acquired a considerable fund of theoretical knowledge. His early letters reveal him reading very widely on architecture, mathematics, chemistry, mechanics, hydrostatics, etc., on which he made copious notes. Thus on 1 February 1796 he wrote to his friend, David Little:⁴

Knowledge is my most ardent pursuit . . . I am now deep in Chemistry – the manner of making Mortar led me to inquire into the Nature of Lime and in pursuit of this, having look'd into some books on Chemistry I perceived the field was boundless – and that to assign reasons for many Mechanical processes it required a general knowledge of that Science. I have therefore had the loan of a MSS Copy of Dr Black's Lectures. I have bought his Experiments on Magnesia and Quick Lime and likewise Fourcroy's Lectures translated from the French by a Mr Elliott. And I am determined to study with unwearied attention until I attain some general knowledge of Chemistry as it is of Universal use in the [practical] Arts as in Medicine.

A year later, he was still 'Chemistry mad' and especially interested in 'Calcareous matters', with the practical objective of securing the best type of cement for building bridges, etc.,

¹ Quoted by Smiles, *op. cit.*, p. 177. Smiles rightly emphasized, however, Smeaton's distrust of pure theory unsupported by practical experiment.

² This view appears to have been based on the statement by Sir David Brewster in the *Edinburgh Review*, October 1839, that 'Telford had a singular distaste for mathematical studies, and never even made himself acquainted with the elements of geometry'; he is even said to have considered that mathematical acquirements unfitted a man for practical engineering. Dr Boucher has recently repeated this view uncritically, in an unfavourable comparison with Rennie (*op. cit.*, p. 30).

³ In *The Story of Telford* (1935).

⁴ Quoted in S. Smiles, *The Life of Thomas Telford* (1867), pp. 128–9, and Gibb, *op. cit.*, p. 10.

both above and below water. But his interests were 'not . . . confined to that alone', and in addition to the works of Black and Fourcroy, previously mentioned, he had read Scheele's *Essays*, Macquer's *Dictionary*, Watson's *Essays*, and writings by his friend Dr Irving [Irvine?]. From these he had taken notes in a pocket-book, 'which I always carry with me,' and into which he had also 'cramm'd Mechanics, Hydrostatics, Pneumatics, and all manner of Stuff, and to which I keep continually adding'.¹

Telford became friendly with various professors at Edinburgh University, such as Professors Stewart, Gregory, Playfair, and Robison, who secured his election to the Royal Society of Edinburgh. Towards the end of his life, in 1827, he was elected FRS, in recognition of his experimental research as well as of his civil engineering achievements. His researches generally had a strong practical bias, such as those on the strength of iron chains and rods for bridge-building, or on the movement of canal boats through water, but, as Sir Alexander Gibb has remarked, they were 'always carried out scrupulously, in logical order and with real scientific care'. Moreover, though he himself lacked a thorough scientific education, he could draw on the scientific help of others, as in 1814 when he carried out experiments on the strength of iron with the assistance of Peter Barlow, Professor of Mathematics at Woolwich Military Academy, who published the results in his book on *The Strength of Materials*.² Telford himself contributed long articles on architecture, bridge-building, and canals to the *Edinburgh Encyclopaedia*. He also wrote a treatise *On Mills* (1798).³ He accumulated a large collection of scientific and technological

¹ Letter to David Little, 27 January 1797, quoted in Smiles, *op. cit.*, p. 134, and Gibb, *op. cit.*, pp. 266-7. This practice of noting down information, gained from reading and observation, in a pocket memorandum book, 'a sort of engineer's vade-mecum', was continued by Telford throughout his life.

² For Barlow, see the *Dictionary of National Biography* (hereafter *DNB*) and also E. G. R. Taylor, *Mathematical Practitioners of Hanoverian England* (1966), pp. 330-1. Fairbairn later carried out similar experiments with the scientific aid of Eaton Hodgkinson. See Musson and Robinson, *Science and Technology*, pp. 77, 117-18, 481, 487. See also *ibid.*, p. 79, and below p. 107, for the earlier experiments of Charles Bage.

³ E. L. Burne (ed.), 'On Mills', by Thomas Telford, *Newcomen Society Transactions*, vol. XVII (1936-7).

books, many of which he donated to the Institution of Civil Engineers when he was elected its first President in 1820. But he always stressed the practical side of engineering, and was scornful of engineering theories untested by experience.

John Rennie's career, as Dr Boucher has emphasized, provides 'a happy blend of theory and practice'.¹ He was educated at Dunbar High School, where he displayed remarkable ability in mathematics and in natural and experimental philosophy, before proceeding to Edinburgh University, where he developed a lifelong friendship with John Robison, the famous Professor of Natural Philosophy. In his youth he is said to have read 'the treatises of Belidor, Parent, and Lambert; Emmerson's *Fluxions and Mechanics*, Switzer's *Hydraulics*, Smeaton's *Experimental Inquiry*, and others'. At the same time he had a thorough practical training under the celebrated millwright, Andrew Meikle, inventor of the threshing machine and the spring sail for windmills. In his subsequent civil and mechanical engineering structures, 'he applied scientific theory, as well as practical experience . . . and used his university training to seek out the root causes of problems which he encountered'. In regard to his water-wheel constructions, for example, Dr Boucher remarks:

It should not be thought that Rennie's application of water power was based on a mere rule of thumb and tradition. He was a practical student of the science of hydraulics, as has been shown by reference to the textbooks he studied. Among his possessions is a handbook on hydraulics in his own writing compiled by himself. Along with the principal formulae are tables incorporating their application.

Rennie also helped John Robison with articles published by the latter in 1797 under the title *Outlines of a Course of Lectures in Mechanical Philosophy*, which then appeared in the third edition of the *Encyclopaedia Britannica*, and were afterwards published separately in four volumes on *Mechanical Philosophy*. It is evident that Rennie had a very good knowledge of structural theory, and Dr Boucher has demonstrated in detail how

¹ In addition to Dr Boucher's recent book on Rennie, previously cited, see also the older life by Smiles, *Lives of the Engineers* (1874), vol. II, in which Rennie's successful combination of theory and practice is similarly emphasized.

he applied this to bridge-building, etc. Rennie was one of the founders of the London Literary and Philosophical Institution, which he attended whenever possible, and he was also a member of the Smeatonian Society. Like Telford, he built up a library rich in works on engineering and other subjects.

Other engineers of this period, such as Thomas Hewes, Peter Ewart, and George Lee in Manchester, were similarly knowledgeable in science and technology.¹ Henry Maudslay, although he started work at twelve years of age, was another remarkable example of 'self-education'. 'He was not merely a mechanical genius wholly ignorant of science; astronomy and the manufacture of telescopes for his own use was his chief hobby. Faraday was his close friend and a frequent visitor to his works. Like many inventors of this period, scientific discoveries were of as much interest to Maudslay as his practical achievements were to the scientist.'²

William Fairbairn, another great self-educated engineer, has left his own account of how he spent his youthful evenings studying arithmetic, algebra, geometry, trigonometry, etc.³ After starting his own engineering business in Manchester, he became a member of the Literary and Philosophical Society, carried out important scientific experiments with Eaton Hodgkinson, the mathematician, on the strength of iron beams and pillars,⁴ was eventually elected a Fellow of the Royal Society, and wrote a considerable number of books on engineering and industrial development.⁵ Other leading engineers

¹ See Musson and Robinson, *Science and Technology*, pp. 98-101.

² Jewkes, *op. cit.*, pp. 46-7. Similar examples of the relationships between scientists such as Faraday, metallurgists, and engineers in the early nineteenth century are provided in the diaries of the Swiss industrialist, J. C. Fischer. See W. O. Henderson, *J. C. Fischer and his Diary of Industrial England 1814-51* (1966), pp. 5, 12, 13, 33, 36-7, 65, 106, 117, 160.

³ W. Pole (ed.), *The Life of Sir William Fairbairn . . . Partly written by himself* (1877), pp. 72-80, 101, 103. See Musson and Robinson, *Science and Technology*, pp. 480-1.

⁴ See *Manchester Literary and Philosophical Society Memoirs*, 3rd ser., vol. V (1831); *British Association*, *Third Report* (1833), *Fourth Report* (1835), *Fifth Report* (1835), *Seventh Report* (1837); *Royal Society Transactions*, 14 May 1840. See Musson and Robinson, *Science and Technology*, pp. 117-18, 481, 487.

⁵ See, for example, his *Useful Information for Engineers* (1856), *The Rise and Progress of Civil and Mechanical Engineering* (1859), *Iron: Its History, Properties and Process of Manufacture* (1861), and *Treatise on Mills and Millwork* (2 vols., 1861-3).

acquired scientific knowledge in much the same way, but they were not all 'self-educated'; Nasmyth, for example, like Rennie, had the benefit of a good academic education.¹ Charles Holtzappfel also appears to have been well educated and 'devoted himself assiduously to the acquirement of scientific and practical knowledge'.²

It was not only in engineering that science was applied. The famous potter, Josiah Wedgwood, was greatly interested in experimental scientific research into clays, glazes, colouring, and temperature control.³ Together with Watt, Boulton, and other industrialists, he associated with scientists such as Darwin, Priestley, and Withering, and was elected a Fellow of the Royal Society. Moreover, in his factory at Etruria, he introduced men who could apply scientific knowledge to pottery manufacture:

Scientific men were engaged, at liberal salaries, in the various departments of the business – in chemistry, in design, in modelling, in painting, etc. The ingenious Mr Alexander Chisholme, who had been employed in experimental chemistry by Dr William Lewis, the celebrated author of the 'Commercium Philosophico-Technicum', was taken into Wedgwood and Bentley's service in 1781, and for many years . . . up to the period of his death, enjoyed the bounty of Mr Wedgwood.⁴

John Marshall, the great Leeds flax-spinner, also had an enthusiasm for natural philosophy, attending scientific lectures and putting his knowledge to practical use:

Before the age of forty his intellectual interests had revolved around the mills and new scientific techniques.

¹ See Musson and Robinson, *Science and Technology*, pp. 489–90.

² *Proceedings of the Institution of Civil Engineers*, 1848, p. 14. Holtzappfel's book on *Turning and Mechanical Manipulation* (3 vols., 1843) 'displays a masterly knowledge of technical art and of the scientific principles underlying it'. *DNB*.

³ R. E. Schofield, 'Josiah Wedgwood and a Proposed Eighteenth-Century Industrial Research Organization', *Isis*, vol. 47 (1956), pp. 16–19; 'Josiah Wedgwood, Industrial Chemist', *Chymia*, vol. 5 (1959), pp. 180–92.

⁴ J. Ward [and S. Shaw], *The Borough of Stoke-upon-Trent* (1843), pp. 433–4. See also S. Parkes, *Chemical Essays* (1815), vol. I, pp. 48–9. For Lewis and Chisholm, see Musson and Robinson, *Science and Technology*, pp. 53–4.

He studied science because it helped him: a knowledge of chemistry was useful in bleaching, the theory of machines in providing power, the properties of materials in construction. Beyond this, he took lecture-notes on optics, electricity, and astronomy. He studied and talked about geology . . . In short he wanted to keep up with the rush of modern knowledge because it could be useful.¹

He was a member of the Literary and Philosophical Society of Leeds, and participated in the founding of a Lancastrian school and Mechanics' Institute in that town; later he proposed the establishment of a university there, and subscribed towards the new university in London, becoming a member of its council.

Professor Rimmer has also shown how Charles Bage, who was associated in business with Marshall, made similar use of applied science. Thus in designing the Castle Foregate mill at Shrewsbury, 'Bage calculated the strength of his mill beams and proved them by full-scale tests. In this, and in other fields like bleaching, he was abreast if not ahead of current developments in science and engineering.'²

Bleaching provides a particularly good example of applied science. The discoveries of the scientists Scheele and Berthollet have already been mentioned. We have found abundant evidence of applied chemistry in the correspondence of James Watt, Thomas Henry (of Manchester), and others, who utilized their chemical knowledge in bleaching, dyeing, and calico-printing experiments.³ As early as the mid-eighteenth century one comes across a reference to chemical analysis of water-supplies preparatory to location of a bleachworks,⁴ while later

¹ W. G. Rimmer, *Marshall's of Leeds, Flax Spinners 1788-1886* (1960), p. 103. See also Musson and Robinson, *Science and Technology*, pp. 153-5, 329-32.

² Rimmer, *op. cit.*, p. 59. Bage combined mathematical theory with experimental testing in ascertaining the strength of iron beams and columns for mill construction. T. C. Bannister, 'The First Iron-framed Buildings', *Architectural Review*, vol. 108 (April 1950), pp. 231-46; A. W. Skempton, 'The Origin of Iron Beams', *Actes VIII International Conference History of Science* (Florence, 1956), vol. 3, pp. 1029-39; H. R. Johnson and A. W. Skempton, 'William Strutt's Cotton Mills, 1793-1812', *Newcomen Society Transactions*, vol. XXX (1955-7), pp. 179-205.

³ See Musson and Robinson, *Science and Technology*, chaps. VII, VIII, IX.

⁴ F. Home, *Experiments on Bleaching* (1756), pp. 281-8; S. Parkes, *Chemical Essays* (1815), vol. IV, pp. 211-12.

on, in the early nineteenth century, one finds the scientist John Dalton providing similar services to industry.¹ A great deal of evidence has also come to light regarding James Watt's collaboration with Dr Joseph Black in the development of synthetic soda manufacture.²

In the development of the steam engine, science also played a considerable role.³ Papin, Savery, and Newcomen owed much to the earlier scientific investigations of such men as Boyle and Huygens. Watt's relations with Black – discoverer of the principle of latent heat – at the time of his development of the steam engine have often been referred to, but Watt's own scientific abilities have been inadequately appreciated. It is abundantly clear, however, that, in the words of his intimate friend, Professor John Robison, he 'was a person of truly philosophical mind, eminently conversant in all branches of natural knowledge'.⁴ He was on very friendly terms, not only with Robison, but with Black, Anderson, and other professors at Glasgow and Edinburgh universities,⁵ and later with Priestley, Darwin, and most other eminent philosophers in England, and also with many French, German, and other continental scientists. He was not merely a brilliant mechanic, but a truly scientific engineer and chemist, well versed in contemporary scientific knowledge and constantly engaged in scientific experiments. He inherited the abilities of his grandfather, a teacher of mathematics, and of his father, a shipwright;

¹ Sir A. J. Sykes, *Concerning the Bleaching Industry* (1925), p. 91. Sykes, of Edgeley, Stockport, 'employed John Dalton, the famous scientist, as consultant on the quality of their water supply'. Parkes, *op. cit.*, vol. IV, pp. 181 ff., strongly emphasized the necessity of chemical analysis of water supplies for bleaching, dyeing, and calico-printing.

² See Musson and Robinson, *Science and Technology*, chap X.

³ See M. Kerker, 'Science and the Steam Engine', *Technology and Culture*, vol. 2 (1961), pp. 381–90; the introduction by A. E. Musson to the new edition of H. W. Dickinson, *Short History of the Steam Engine* (1963); D. S. L. Cardwell, *Steam Power in the Eighteenth Century* (1963).

⁴ Article on 'Steam Engine', *Encyclopaedia Britannica*, 3rd ed. (1797). Robison's articles on steam and steam engines were later reprinted, with notes and additions by Watt (Edinburgh, 1818). See also F. Arago, 'James Watt', in *Biographies of Distinguished Scientific Men* (English trans., 1857); J. P. Muirhead, *The Life of James Watt*, 2nd ed. (1859); G. Williamson, *Memorials of . . . James Watt* (1856).

⁵ See Musson and Robinson, *Science and Technology*, pp. 179–80, for Watt's relations with Anderson.

educated at Greenock Academy and trained as an instrument-maker, he early exhibited strong interests in mathematics, mechanics, and chemistry – before he was fifteen he had read Gravesande's *Mathematical Elements of Natural Philosophy*, translated by Desaguliers. Later, when appointed instrument-maker to Glasgow University, he started learning German so that he might read Leupold's *Theatrum Machinarum*, and Italian for a similar purpose; to construct an organ, he read the *Harmonics* of Dr Robert Smith, of Cambridge. Before his famous improvements on the steam engine, he had read Desaguliers and Belidor on this subject, and these improvements were the outcome of careful experiments on steam and on models of Savery and Newcomen engines, in which he consulted with Professor Black. His famous tea-kettle, usually dismissed nowadays as mythical, was in fact, as his diary reveals, used as a miniature boiler in a series of laboratory experiments, and he produced tables on the thermal efficiency of steam engines, based on mathematical theory as well as practical experiment. He also appears to have rediscovered the principle of latent heat, independently of Black.¹ Watt himself, of course, like Boulton, Wedgwood, Keir, and other industrialists, was made a Fellow of the Royal Society.

That science could make useful contributions to early steam engineering is also shown by other evidence. The scientist Davies Gilbert (or Giddy), for example, provided Jonathan Hornblower, the practical engineer, with a great deal of advice in regard to the latter's attempts to develop his compound and 'rotary' (turbine) engines.² Similarly, he rendered valuable assistance to Richard Trevithick, inventor of the high-pressure, non-condensing steam engine.³

In many ways, then, applied science was helping to bring about the Industrial Revolution. On the other hand, one must not fall into the error of supposing that the latter was simply

¹ On this latter point, see D. Fleming, 'Latent Heat and the Invention of the Watt Engine', *Isis* vol. 43 (1952), pp. 3–5.

² A. C. Todd, 'Davies Gilbert – Patron of Engineers (1767–1839) and Jonathan Hornblower (1753–1815)', *Newcomen Society Transactions*, vol. XXXII (1959–60), pp. 1–13.

³ H. W. Dickinson and A. Titley, *Richard Trevithick, the Engineer and the Man* (1934), p. 36 *et passim*.

a product of the Scientific Revolution. . . . In the early mechanization of the cotton industry, applied science appears to have played a very minor role.¹ Inventors such as Kay, Paul, Wyatt, Hargreaves, Arkwright, Crompton, and Cartwright appear to have had little or no scientific training, though they often utilized the knowledge and skills of clock- and instrument-makers. They have fairly recently been described as 'mostly . . . men of some social standing and good education',² but this is questionable. Kay, Hargreaves, and Arkwright, for instance, appear to have had a very limited and rudimentary schooling before being put to a trade, and Wyatt, though educated at Lichfield Grammar School, became a carpenter. Cartwright, it is true, was a Fellow of Magdalen College, Oxford, but he was a Doctor of Divinity and was apparently unacquainted with mechanical matters prior to his invention of the power-loom. Crompton, however, is an interesting case. He had a good schooling in writing, arithmetic, book-keeping, geometry, mensuration, and mathematics, under an outstanding master, William Barlow of Little Bolton, and during his later 'teens he continued his education by attending evening-school, improving his knowledge of algebra, trigonometry, and mathematics generally.³ According to French, his biographer, his invention of the mule 'appears to have been the result of pure inductive philosophy, followed out step by step with a mathematical precision for which his mind had been duly prepared by previous education'.⁴

Sometimes one comes across contemporary scientists and industrialists deploring the general lack of applied science in industry. Theophilus L. Rupp, for instance, a German who

¹ As was pointed out by Arkwright's Counsel during the great patent case of 1785, in oft-quoted words: 'It is well known that the most useful discoveries that have been made in every branch of art and manufacture have not been made by speculative philosophers in their closets but by ingenious mechanics, practically acquainted with the subject matter of their discoveries.'

This statement may be contrasted, however, with that of defending Counsel in the Tennant bleaching patent case of 1802, that 'most of these discoveries arise from scientific men engaging in them, for the purpose of science and speculation in their closet'.

² Jewkes, *op. cit.*, pp. 44-5.

³ G. J. French, *The Life and Times of Samuel Crompton* (1859), pp. 30, 32.

⁴ *Ibid.*, p. 53.

settled in Manchester as a cotton manufacturer in the late eighteenth century, wrote as follows.¹

The arts [manufactures], which supply the luxuries, conveniences, and necessities of life, have derived but little advantage from philosophers. . . . In mechanics, for instance, we find that the most important inventions and improvements have been made, not through the reasonings of philosophers, but through the ingenuity of artists [craftsmen], and not unfrequently by common workmen. The chemist, in particular, if we except the pharmaceutical laboratory, has but little claim on the arts: on the contrary, he is indebted to them for the greatest discoveries and a prodigious number of facts, which form the basis of his science. In the discovery of the art of making bread, of the vinous and acetous fermentations, of tanning, of working ores and metals, of making glass and soap, of the action and applications of manures, and in numberless other discoveries of the highest importance, though they are all chemical processes, the chemist has no share. . . . The art of dyeing has attained a high degree of perfection without the aid of the chemist, who is totally ignorant of the rationale of many of its processes, and the little he knows of this subject is of late date.

It is significant, however, that Rupp went on to refer warmly to the recent experimental and theoretical work of Thomas Henry of Manchester, in dyeing, and of the French chemist, Berthollet, in bleaching. And it is also significant that Rupp himself was described by his fellow-countryman, P. A. Nemnich as

a German, who possesses great knowledge of chemistry, and who at the same time, as a manufacturer, can carry out most splendid experimental applications for the benefit of scientific knowledge . . . Mr Rupp at the time of my visit to Manchester was very busy with the building of a big

¹ T. L. Rupp, 'On the Process of Bleaching with the Oxygenated Muriatic Acid', *Manchester Literary and Philosophical Society Memoirs*, vol. V, part i (1798).

spinning mill to be erected according to his own ideas, and many improvements are expected from him.¹

Rupp was a member of the Manchester Literary and Philosophical Society, to which he read several papers, including a notable one on chlorine bleaching and another making an anti-phlogiston attack on Priestley and defending 'the new chemical theory', with numerous references to the works of foreign chemists such as Lavoisier, Bergman, etc.² William Henry, second only to John Dalton as a scientific chemist in Manchester in this period,³ referred to some of his experiments having been witnessed by 'my friend Mr Rupp . . . who is much conversant in the observation of chemical facts'.⁴

Like Rupp, however, William Nicholson also referred to the many matters on which chemists were ignorant, or which they could not adequately explain.⁵ The great controversy over phlogiston and the new chemical theory, he said, 'amounts to a confession of ignorance in our theoretical explanation'. But he pointed out that continuous experimental investigation was leading to the discovery of the laws of nature, and that one must expect the 'successive emendation' of scientific theories, and 'the rejection of principles formerly held to be essential to the science' of chemistry. Nicholson himself did much to spread a knowledge of chemical theories and facts, by his own works and by his translations of those of foreign chemists.

In engineering, too, one comes across similar remarks emphasizing the role of empiricism. George Atwood, for instance, Fellow of Trinity College, Cambridge, and lecturer in natural philosophy, pointed out inconsistencies in the theory

¹ P. A. Nemnich, *Beschreibung einer in Sommer 1799 von Hamburg nach und durch England geschehenen Reise* (1800), p. 324.

² *Manchester Literary and Philosophical Society Memoirs*, vol. V, part i (1798).

³ See Musson and Robinson, *Science and Technology*, pp. 246-50.

⁴ *Philosophical Transactions* (1797), p. 410. Manchester Central Reference Library possesses a copy of Henry's *General View of the Nature and Objects of Chemistry* (1799) inscribed to 'Mr Rupp from the Author'.

⁵ See, for example, his remarks in his *Dictionary of Chemistry* (1795), pp. iii-v and 1-2. He had previously published *An Introduction to Natural Philosophy* (1782) and *The First Principles of Chemistry* (1790), both of which ran into several editions. Later he started *Nicholson's Journal of Natural Philosophy, Chemistry and Arts*, an important scientific periodical. See *DNB* and Hans, *op. cit.*, pp. 115 and 158.

of motion and doubted whether it could provide any assistance to the practical mechanic in constructing power-driven machinery. 'Machines of this sort owe their origin and improvement to other sources: it is from long experience of repeated trials, errors, deliberations, [and] corrections, continued through the lives of individuals, and by successive generations of them, that sciences, strictly called practical, derive their gradual advancement . . .'¹

Peter Ewart, however, an outstanding Manchester engineer of that day, well versed in engineering science,² though regretting 'that theory should appear to be at variance with practice', thought that Atwood had 'pressed his argument too far'.³ There was no doubt 'that ingenious men, of rare natural endowments, have, without any scientific aid, accomplished wonders in the invention and improvement of machinery', but there were also some notable examples to the contrary in 'the history of useful discoveries in mechanics'.

If Huygens and Hooke had not been scientific as well as ingenious men, we might possibly have been still ignorant of the properties of the balance regulated by springs. If Smeaton had not availed himself of just theory, as well as experiment, we might still have had to learn the principles by which we must be guided in applying water to the best advantage as a moving power. If a clear and strong understanding, and a mind richly stored with scientific attainments, had not been combined with wonderful fertility of invention in the justly celebrated improver of the steam engine [Watt]; incalculable labour might still have been wasted in performing operations which are now accomplished with as much ease and regularity as the gentle motions of a time-piece.

Similarly, in regard to the application of chemical science, one frequently comes across contemporary statements of its importance. Thus we find William Henry putting forward

¹ G. Atwood, *A Treatise on the Rectilinear Motion and Rotation of Bodies* (1784), p. 381, quoted by P. Ewart, 'On the Measure of Moving Force', *Manchester Literary and Philosophical Society Memoirs*, 2nd ser., vol. II (1813), p. 111.

² See Musson and Robinson, *Science and Technology*, p. 99.

³ Ewart, *op. cit.*, pp. 112-14.

A General View of the Nature and Objects of Chemistry, and of its Application to Arts and Manufactures (Manchester, 1799), in which, after quoting Bacon in support of 'the union of theory with practice', he referred to the illustrious example of Watt and Wedgwood, 'both of whom have been not less benefactors of philosophy, than eminent for practical skill', and went on to demonstrate the utility of chemistry in a wide range of industries – in metallurgy, in the production of alkalis, acids, etc., in the glass and pottery manufactures, in brewing, in bleaching, dyeing, and calico-printing.

Some parts of the natural philosophy of the eighteenth century, as of the seventeenth, had no immediate practical application. Though in some cases they were ultimately to have revolutionary industrial consequences, they were originally developed mainly out of intellectual curiosity, out of a desire to unravel the mysteries of nature. Electricity and magnetism, for instance, which were very popular subjects in contemporary lecture courses, played little or no part in the early Industrial Revolution. 'Electricity,' said the German traveller, C. P. Moritz, 'is the plaything of the English.'¹ But other subjects, such as chemistry, mathematics, mechanics, hydraulics, and hydrostatics. . . . certainly were studied for utilitarian as well as scientific reasons. Even the most purely philosophical investigations sometimes had very practical connexions and consequences. Priestley, for example, says that he began his researches into 'airs' or gases as a result of living in Leeds next to a brewery, where he noticed that 'fixed air' (carbon dioxide) was evolved in the fermentation vats;² his investigations led immediately to the manufacture of artificial mineral waters,³ while their long-term consequences were of far wider importance.

¹ C. P. Moritz, *Journeys of a German in England in 1782* (trans. and ed. by R. Nettel, London, 1965), p. 70.

² J. Priestley, *Experiments and Observations on Different Kinds of Air*, vol. II (1775), pp. 269–70; F. W. Gibbs, *Joseph Priestley* (1965), pp. 57–60.

³ See Musson and Robinson, *Science and Technology*, pp. 234–9.

3 Some Statistics of the Industrial Revolution in Britain

T. S. ASHTON

[The following extracts are taken from the article by T. S. Ashton in *The Manchester School*, vol. XVI, 1948, pp. 214-34.]

The years between 1760 and 1830 saw a series of changes in British life so spectacular as to lead historians to attach to the period the somewhat misleading title of the Industrial Revolution. Early observers, impressed by the developments associated with the names of Arkwright, Crompton, Watt, Cort, Stephenson, and many others, tended to regard the technical innovations as the hinge on which all else turned. It was only later that scholars began to ask why these men of invention appeared just when they did. Historians, obsessed as most of them are with the affairs of government, were at first disposed to give the answer in terms of policy. Some attributed the outbursts of invention and enterprise to the action of enlightened rulers who, they believed, had built up a powerful mercantilist society in England, with widespread connexions overseas: the inventions and the new methods of organization were the response of industry to the demands of trade. Others argued that it was not positive measures of statecraft, but the gradual decline of attempts at regulation and stimulus that threw open the door to innovation. One group of writers, looking at the social and religious affiliations of the industrialists, finds the source of the technical changes – and much else besides – in seventeenth-century puritanism and eighteenth-century nonconformity. Another, pointing out that the changes in technique were, in fact, less sudden than had been imagined, presents them as the fruit of a tree springing from the work of Newton, Bacon, and still earlier scientists. And yet another school of writers treats the whole movement as a

product of the new systems of speculative thought and political theory to which the eighteenth century gave rise.

It is not my purpose to pass judgement on these interpretations of the Industrial Revolution. Every historical event is the result of everything that happened before; and there is, no doubt, some measure of truth in all of them. But historical relationships may be either close or remote – in space, in time, and in logic. If we wish to ascertain the degree of relationship between changes of one kind and those of another we must make use of statistical methods: today almost every branch of historical studies is becoming increasingly statistical. There is, let it be admitted, some danger in this development. For since economic phenomena are more susceptible of measurement than political or social or intellectual phenomena there may be a tendency to overstress their importance. Some recent writers – not all of them Marxists – have gone so far as to attribute the changes in politics, art, and religion, to the development of industrial technique or of ‘production functions’. Schumpeter (who professes to see capitalism alike in the canvases of El Greco and in the evolution of the lounge suit) treats the Industrial Revolution as one of the long waves which he believes to be a characteristic of capitalistic progress; and Kondratieff, who has expressed this determinist doctrine in extreme form, suggests, not merely that the wars and revolutions of the period had economic aspects, but that they were the direct and inevitable result of pressures generated by the long wave.

I have no wish to enter these regions of speculation [in this paper]. I am content to express the not very original opinion that the *proximate* causes of the Industrial Revolution were economic. Under whatever favouring conditions of policy, creed, or scientific thought, there took place in the eighteenth century a vast increase of natural resources, labour, capital and enterprise – of what the economist calls the factors of production. In the early stages it was the growth of capital that was of chief significance. At the beginning of the century the Government itself had been obliged to pay 7 or 8 per cent for money, and but for the operation of the usury laws industrialists would have had to pay considerably more than this. Since,

however, people were prohibited from offering more than 5 per cent, and evasion of the law was expensive, some of them, it appears, had to go without the resources they needed. When, fifty years later, the yield on Consols was down to 3 or less, and 4 per cent was the accepted long-term rate over a wide field of business, all sorts of projects that had been previously impossible came into the open. It would, no doubt, be a mistake to interpret the financial trends of the eighteenth century in terms of twentieth-century conditions. There was, of course, no organized capital market. The typical firm was a partnership which obtained its resources largely by the reinvestment of profits; and the amount ploughed back, it seems likely, was influenced very little by changes in the rate of interest. Outside ordinary industry, however, were the chartered companies, the turnpike trusts, the river and canal and dock undertakings and other public utilities. When money was cheap these tended to increase the scope of their operations; investment here, we may assume, increased incomes in the community in general, and so increased the demand for the products of industry in the narrow sense. In this way the fall in the rate of interest played a part (the importance of which has not, I think, been properly appreciated) in the industrial expansion.

Innovations means much more than technical invention: it includes (as Schumpeter says) changes in markets, in methods, and in the proportions in which factors are combined to attain a given end. It is not possible to measure such changes precisely, and even the course of invention in the narrow sense is not easy to chart. Much technical improvement takes place behind the scenes: in the eighteenth century, in particular, there was some reluctance to make public even such a hazy sketch of the nature of a device as was necessary for a specification. Nevertheless, the number of patents taken out may serve perhaps as a rough index of innovation. The figures in Table I show, as we should expect, a strong upward trend, but they show also the cyclical variations typical of most economic data. There are significant peaks in the figures for 1766, 1769, 1783, 1792, 1801-2, 1813, 1818 and 1824-5 – nearly all of them years when the rate of interest was low, and when, as we have evidence, industry and trade were active. (1813 is the only one

of the series which cannot be described as a year of boom.) It may reasonably be objected that many of the discoveries may possibly have been made earlier but were held back until times were more propitious. Kondratieff has argued (though without much supporting evidence) that most major inventions are made during periods of recession and applied during the ensuing periods of recovery. But at least, the fact that so many patents were taken out in years of prosperity, and so few in years of depression (such as 1775, 1788, 1793, 1797, 1804, 1817, 1820, and 1826), suggests that it was the hope of gain, rather than of avoiding loss, that gave the impulse. It may also be objected that many patents were taken out by men whose hopes outran their ingenuity or practical sense, and that the high figures of the booms represent not solid progress but the mere blowing of bubbles. A glance at the names of the patentees in each of the years of high activity suggests, however, that there is something more in it than that. The list includes, for 1769, Arkwright, Watt, and Wedgwood; for 1783, Cort, Onions, and Bramah; for 1792, Wilkinson, Cartwright, and Curr; for 1801-2, the Earl of Dundonald, Trevithick, and Symington; for 1813, Horrocks; for 1818, Brunel and Mushet; and for 1824-5, Maudslay, Roberts, and Biddle. One could write a fairly complete history of technology for this period without mention of any other names than these.

The chronology of patents is a field that would repay further study. Sir Arnold Plant, who has surveyed the later history of patents (from 1854) makes the interesting suggestion that the inventive faculty is aroused by any sudden change in the trends of prices, wages, or costs.¹ On such matters we have for the period of the Industrial Revolution only very scanty data. But it may be noticed that the peaks in the figures of patents in 1766, 1783, 1802, and 1818 came (at varying intervals, it is true) after the transition from conditions of war to those of peace, with, in each case, some disturbance of relative prices. And the outstanding booms in patents of 1792, 1802, and 1824-5 came at the end of periods when the rate of interest had been falling significantly. It may be, indeed, that both the

¹ 'The Economic Theory Concerning Patents for Inventions', *Economica*, N.S., vol. I, no. 1 (1934).

TABLE I

<i>Year</i>	<i>Number of Patents</i>	<i>Yield on Consols</i>	<i>Year</i>	<i>Number of Patents</i>	<i>Yield on Consols</i>
1756	3	3·4	1794	55	4·4
7	9	3·4	5	51	4·5
8	14	3·2	6	75	4·8
9	10	3·6	7	54	5·9
60	14	3·8	8	77	5·9
1	9	3·9	9	82	5·1
2	17	4·3	1800	96	4·7
3	20	3·4	1	104	4·9
4	18	3·6	2	107	4·2
5	14	3·4	3	73	5·0
6	31	3·4	4	60	5·3
7	23	3·4	5	95	5·0
8	23	3·3	6	99	4·9
9	36	3·5	7	94	4·9
70	30	3·6	8	95	4·6
1	22	3·5	9	101	4·6
2	29	3·3	10	108	4·5
3	29	3·5	1	115	4·7
4	35	3·4	2	118	5·1
5	20	3·4	3	131	4·9
6	29	3·5	4	96	4·9
7	33	3·8	5	102	4·5
8	30	4·5	6	118	5·0
9	37	4·9	7	103	4·1
80	33	4·9	8	132	3·9
1	34	5·2	9	101	4·2
2	39	5·3	20	97	4·4
3	64	4·8	1	109	4·1
4	46	5·4	2	113	3·8
5	61	4·8	3	138	3·8
6	60	4·1	4	180	3·3
7	55	4·1	5	250	3·5
8	42	4·0	6	141	3·8
9	43	3·9	7	150	3·6
90	68	3·9	8	154	3·5
1	57	3·6	9	130	3·3
2	85	3·3	30	180	3·5
3	43	4·0			

secular and cyclical falls of interest rates explain why most of the inventions of the period were directed to the economy of labour rather than of natural resources or of capital. But this is speculation. It is at least clear that, whatever the nature of the connecting thread, the inventions were not a force operating more or less casually from outside the system, but were an integral part of the economic process.

The record of patents tells only a part of the story. Innovation was not simply a matter of the introduction of a number of devices at particular points of time: it was a continuous process. Details of the day-to-day adjustments of machinery, of the innumerable petty economies of materials, of the gradual training of labour, and so on, are hidden from us. But their results can be read in the statistics of output. . . .

4 *The Natural History of Industry*

CHARLES C. GILLISPIE

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I

In a previous paper, the problem of science and industrialization was pursued in some detail through an account of the discovery of the Leblanc process. The present paper approaches the question on a broader plane and ventures to offer some general considerations. Both articles are a result of investigations pursued in France, and refer specially, therefore, to the pattern of French scientific and industrial development. This was necessarily affected by certain factors peculiar to French history, notably the ever-growing centralization of cultural development in Paris and its learned bodies and the profound instinct of everyone concerned – scientist and statesman, industrialist and artisan – to expect of a paternalistic state the impetus which their British counterparts drew from private venture and expected only of themselves. But these are only social influences. Neither science nor industrialization has ever been national in scope, and to consider a question related to the Industrial Revolution in a framework other than British may have the merit of modifying the tendency to see this great event in the exclusive perspective of the Midlands of England and the Lowlands of Scotland.

In histories of eighteenth-century and imperial France, the assertion is often encountered that science was revolutionizing manufacturing, and Napoleon's encouragement of this process is frequently described as a major reason for the success of his industrial policy until the crisis of 1810. Historians have obviously drawn this judgement from contemporary writings by scientists themselves. That science ought to receive public

credit for the unprecedented progress of the arts since the 1770s or 1780s is perhaps the most protean reflection in the technological literature of the time. Of chemistry, for example, Chaptal writes:

On l'a vue donner de nouvelles méthodes pour le blanchissage des toiles; fabriquer, de toutes pièces, le sel ammoniac, l'alun et les couperoses; décomposer le sel marin pour en extraire la soude; enrichir la teinture de nouveaux mordans; former le salpêtre et le raffiner, par des procédés plus simples; composer la poudre par des méthodes plus promptes et plus sûres; réduire le tannage des peaux à ses vrais principes, et en abrégier l'opération; perfectionner l'extraction et le travail des métaux; simplifier la distillation des vins; rendre les moyens de chauffage plus économiques; établir la combustion de l'huile et l'éclairage de nos habitations sur de nouveaux principes, et nous fournir les moyens de nous élever dans les airs et d'aller consulter la nature à trois ou quatre mille toises audessus de nos têtes.¹

No one, it is true, specifies exactly how science was changing the face of industry. But knowledge is power; yesterday's banality is today's source material; and on the basis of all this authoritative testimony, the modern historian has very naturally supposed that theoretical science exerted a fructifying and even a causative influence in industrialization. The proposition is inherently persuasive, and there must be something in it.

II

The problem, however, is to know what there is in it. For if the question be approached in detail – as for example in my study of the origin of the Leblanc process – it proves extraordinarily difficult to trace the course of any significant theoretical concept from abstract formulation to actual use in industrial operations. The objection might be raised that such an approach loses sight of the forest for the trees. But in this case no coherent pattern emerges from the widest perspective or the most distant focus. Consider the more significant achieve-

¹ J. A. C. Chaptal, *Chimie appliquée aux arts* (3 vols.; Paris, 1807), vol. I, p. xiv.

ments in basic science – the progress of taxonomy in botany and zoology, the theory of combustion, the foundation of exact crystallography, the discovery of the electric current, the extension of the inverse-square relationship to magnetic and electrostatic forces, the analytical formulation of mechanics, the resolution of the planetary inequalities – and range these accomplishments side by side in the mind's eye with the crucial points of technological advance in the Industrial Revolution – deep ploughing and crop rotation, the development of power-driven textile machinery and factory production, the discovery of coke and its substitution for charcoal in smelting ores, the improvement of the steam engine by the separate condenser and the sun-and-planet linkage, the puddling process for the conversion of iron to steel – and it is immediately evident that no apparent relationship existed between these two sets of achievements except the vague and uninteresting one that both occurred in a technical nexus. In a recent book Mr and Mrs Clow have demonstrated with great force that the rapid expansion of chemical industry must henceforth be numbered among the basic factors in industrialization.¹ But although the chemical industry was no doubt closer to science than any other, their discussion does not suffer in the slightest from the fact that its references to the revolution in theoretical chemistry are fleeting – to have brought this into an account of Scottish industry would have been to introduce an extraneous element. And this excellent work does not illuminate the question, therefore, as indeed it was not intended to do; in fact the authors fall into the altogether trifling error of repeating the legend about Watt's invention of the separate condenser having been a practical outcome of Black's formulation of the principle of latent heat.² Ultimately, then, one is tempted to fall back upon a generalization of L. J. Henderson's much quoted dictum to the effect that science is infinitely more indebted to the steam engine than is the steam engine to science. But this is too easy a way out, for repeating this famous remark does not conjure away the sources. It cannot very well

¹ A. and N. Clow, *The Chemical Revolution* (London, 1952).

² Ibid., p. 590; cf., Donald Fleming, 'Latent Heat and the Invention of the Watt Engine', *Isis*, vol. 43 (1952), pp. 3–6.

be supposed that men of the eminence of Chaptel, Lavoisier, Cuvier, and many others had no more in mind, in their frequent references to the utility of science, than to win for it public esteem, and that no real substance lay behind their belief in its practical contribution to the arts.¹

Neither does it clarify the issue to turn to economic history. For, on the one hand, the sources do not exhibit a steady movement forward in French industry, and on the other hand, except in the case of chemistry, no correlation is to be discerned between the areas of greatest progress in industry and in science. More intensive scientific attention was devoted, for example, to the extractive and metallurgical industries than to any other. To cite only the most obvious illustrations, Gabriel Jars's splendid *Voyages métallurgiques* was published between 1774 and 1781;² in 1788, Berthollet, Vandermonde, and Monge printed a very lucid memoir on iron and steel;³ and in 1794, a further treatise by Monge was the most solid contribution to the technological series commissioned by the Committee of Public Safety in order to stimulate war production.⁴ But what practical effect all this study had is far from

¹ Though outdated, the most comprehensive account of technological history is still Charles Ballot, *L'Introduction du machinisme dans l'industrie française* (Paris, 1923). To mention only the most obvious printed materials, Ballot may be supplemented with G. and H. Bourgin, *L'industrie sidérurgique en France au début de la Révolution* (Paris, 1920); Odette Viennet, *Napoléon et l'industrie française* (Paris, 1947); A. L. Dunham, *La Révolution industrielle en France* (Paris, 1953); Henri Sée, *Histoire économique de la France*, vol. II (Paris, 1951); *Documents relatifs à la vie économique de la Révolution* (5 vols., Paris 1906–10), a series which in 1911 became *Bulletin D'histoire économique de la Revolution* (5 vols., 1912–19); and among older works: J. A. C. Chaptal, *De l'industrie française* (2 vols., Paris, 1819); C. A. Costaz, *Essai sur l'administration de l'agriculture, du commerce, des manufactures, et des subsistances* (Paris, 1818); the *Bulletin de la Société d'Encouragement* (from 1801); the *Journal des Mines* (from 1794); and the *Annales de Chimie* (from 1789).

² (3 vols.; Lyon, 1774–81). Published posthumously, these memoirs describe the iron industries of Germany, Sweden, Norway, England, and Scotland which Jars had studied between 1757 and 1769 in a series of expeditions originally undertaken at the instance of Trudaine.

³ 'Mémoire sur le Fer', *Mémoires de l'Académie royale des sciences* (1786), pp. 132–200.

⁴ *Description de l'art de fabriquer les canons* (Paris, 1794). Monge begins this treatise with a little disquisition on the composition of the atmosphere and the occurrence of iron as the oxide, but his description of smelting procedures does not, of course, differ in any essential from that given in the 1786 *Mémoire* (note 3), in which what iron ore is deprived of is its dephlogisticated air (loc. cit., p. 133).

clear. The metal trades proved the least resilient in recovering from the drastic technological setbacks dealt to French industry by the revolutionary disturbances. There is some doubt whether rehabilitation was complete even by 1815.¹ All through this period, inspectors of the *Agence des mines* were very fretful about the reluctance of iron masters to disturb themselves by shifting to coke; and with good reason, for at the end of the Napoleonic wars the only foundry in all France using coke was the furnace established at Le Creusot, which had gone into operation in 1782.² In any case, if it be agreed that the central feature of industrialization was the development of factory production, the crucial role must clearly be ascribed to the textile industries, and one searches their history in vain for any trace of scientific influence, except in the bleaching or dyeing of the finished product. In textile manufacturing – and even in metallurgy – French entrepreneurs were shown the way, not by scientific research, but by Englishmen and Scotsmen. John Holker's establishment at Rouen is the most famous example, but judging from the number of traces which his compatriots have left behind them in the commercial sources at the *Archives nationales*, there must have been literally hundreds of British artisans selling eagerly sought skills and ingenuity in France in the latter part of the eighteenth century.³ Many remained even through the 1790s. Yet despite the ambiguities just discussed, and notwithstanding the economic disarray of the 1790s, this is precisely the decade from which Cuvier, for example, illustrated the benefits conferred by science on the arts.⁴ The literature, therefore, exhibits real contradictions, and it is with this in view as the problem to resolve that the remainder of this paper is addressed to the question of what sort of influence science actually did exert in manufacturing.

¹ Ballot, *op. cit.* pp. 494–527.

² Dunham, *op. cit.* p. 78.

³ André Rémond, *John Holker* (Paris, 1946).

⁴ Georges Cuvier, *Rapport historique sur les progrès des sciences naturelles* (Paris, 1808).

III

In approaching the problem, it will be convenient to recall the history of the Leblanc discovery, which resists with great firmness being fitted into either of the extreme views that are sometimes expressed about the historical relations between science and invention. It cannot be used as an illustration of the austere doctrine that there was no real connexion – Leblanc was closely associated with leading members of the scientific community, and it is a revealing fact that every one of the processes for making soda invented in France between 1775 and 1800 was verified and evaluated by one of a succession of three scientists, Macquer, Berthollet, and Darcet, who among them were informed about every development and exercised, therefore, a measure of control over the field. (This indeed was typical of the relationship, if not between science and the arts, at least between scientists and artisans, which obtained in eighteenth-century France and which lends to the phrase ‘Science governs the Arts’ a literal sense not always appreciated, one that goes far to explain the rebellion of the artisans’ societies against the *Académie des sciences* at the time of the Revolution and the enmity of the popular clubs for science.)

On the other hand, neither does the Leblanc invention bear out the contention that there is no essential difference between science and applied science. Leblanc seems to have found his process, not through some flashing theoretical insight, but by means of a fallacious analogy with the smelting of iron ore. Not only so, but after he had worked it out, neither he nor any of the other artisans interested in alkali production made any attempt to investigate or explain the nature of the reactions involved. They concentrated their efforts – though for a long time with no success – on trying to make money by one method or another – in Leblanc’s case by first persuading the Government to subsidize him.

Instead, therefore, of substantiating a doctrinaire interpretation of the relations between science and industry, the history of the Leblanc discovery suggests rather that the two main departments of technical activity are distinct but related. It is a relationship which may, perhaps, be observed quite

generally throughout the technical history of industrialization – it could equally well be illustrated by a detailed history of any of the innovations credited to chemical science by Chaptal in the passage quoted in the second paragraph of this paper, as well as in many others of those eighteenth-century inventions which owed anything at all to scientific activity.¹ And there may even be discerned a certain structure in the relationship. For schematically there were two phases through which science moved in becoming effective in industry; first, the exploitation of science by inventors, artisans, and industrialists, and second (often simultaneously), the conscious application of science to practical problems.

IV

The first phase is precisely illustrated in the activities of Carny, Malherbe, Guyton de Morveau, and Leblanc's other fore-runners, whose processes for making soda were more scientific and ultimately less successful than his – more scientific in that instead of being discoveries, they were attempts to draw advantage from reactions already known to chemical science. This practical exploitation of scientific knowledge may also be exemplified by the telegraph, the origins of military aviation, the emergency production of saltpetre, and indeed by most of the items which, like these, are cited by Pouchet, Despois, and even Mathiez in their jejune portrayals of the great revolution as favourable to scientific activity.² Actually, of course, these accounts simply adopt the Jacobin view which found value in science only in so far as its utility could be demonstrated. In fact, however, the distinction between this sort of thing and science is obvious and elementary. It can even be brought down to the level of persons, where the difference between the scientist and the practical man is basically a matter of temperament.

The point was discussed by Lavoisier, when in his forlorn

¹ See, for example, Cuvier, pp. 345–58, for an enumeration of innovations attributed to scientific progress.

² G. Pouchet, *Les sciences pendant la terreur* (Paris, 1896); Eugène Despois, *Le Vandalisme révolutionnaire* (Paris, 1868); Albert Mathiez, 'La mobilisation des savants en l'an II', *Revue de Paris* (November–December 1917), pp. 542–65.

defence of the *Académie des sciences*, he was trying desperately to preserve the independence of science, of free inquiry, from the Jacobin passion for assimilating it to the useful arts. However useful to industry science may be, wrote Lavoisier – and he thought it very useful indeed – the spirit which moves the scientist is fundamentally unlike that which animates the artisan. The scientist works for love of science and to increase his own reputation. When he makes a discovery, he is eager to publish it, and his object is only to secure his intellectual property in his achievement. The artisan on the other hand, whether in his own research or in using the research of others, is always thinking of his economic advantage. He publicizes only what he cannot keep secret and tells only what he cannot hide. Society benefits both from the disinterested investigation of the *savant* and the interested speculation of the artisan. Confound the two, however, and both will lose the spirit distinctive to them.¹

Already in the eighteenth century, France was playing Greece to the modern world, and men of learning clearly and instinctively distinguished between the domains of science and practice. Throughout its history, the *Académie des sciences* had had two duties: to advance the sciences and to act as a panel of experts who evaluated the projects of aspirants for the favour offered by the state in its perennial effort to encourage French industry. And the very procedures of the *Académie* as recorded in its *registres* make it evident that disinterested research was the main interest of the foremost members, and that technological *expertise* was at best their corporate duty.² To say that they despised the latter would strain the classical analogy. On the contrary, they regarded it as a just and important obligation, if sometimes an onerous and tiresome one; but however important to society, the realm of practice belonged to a different and a lower order of consideration than the realm of theory and abstraction and advancement of the understanding of nature.

In this attitude, French scientists were more severe, perhaps,

¹ Lavoisier to Lakanal, *Œuvres de Lavoisier*, (6 vols.; Paris, 1864–93), vol. IV, p. 623.

² Unfortunately the *procès-verbaux* of the academy remain unpublished. They may be consulted, however, at the *Archives de l'académie des sciences*, at the *Institut de France*.

than their colleagues in other countries and particularly in Great Britain. Accompanying this paper, there is another by Dr Robert E. Schofield, who offers an account of his most meticulous researches into the industrial orientation of science in the Lunar Society circle. And in a large sense Dr Schofield's subject would appear to be a further illustration of that mutual stimulation of science, commercial enterprise, and Puritan influence (now softened into Nonconformity or Unitarianism), successive manifestations of which so forcibly strike students of British social, intellectual, and scientific history from the seventeenth through the nineteenth centuries. But there is perhaps an underside to the coin of this famous correlation. For technical activity is one thing, but power of abstract thought is another; and it may well be wondered whether a certain vulgarity in this British utilitarianism – thoroughly evident, for example, in Bacon – was not responsible for the relative poverty of British achievement over the centuries in the abstract reaches of scientific thought; and further whether the French instinct to separate thought and practice, while giving each its due, was not by the same token responsible for the formal elegance and intellectual eminence of French scientific leadership in its great days.

But however that may be in general, it is at least notable that after the mobilization of talent in the year II, the men who were truly scientists – Berthollet, Monge, Fourcroy, and Vauquelin, for example – moved back from war service to science, whereas those whose instincts and interests lay in production and enterprise – Carny, Deyeux, Pelletoer, Chaptal, and many others – moved outwards in the favourable climate of Thermidor towards the exploitation of whatever opportunities their war service had suggested to them. It is true that, on the whole, the scientists had served the State in a higher advisory capacity. They saw the problems and possibilities more clearly and steadily, and they had prestige. But it is an illusion that theoretical science was applied to war production, even in the terrible emergency of the year II. Science was only exploited. What was applied was scientists.

V

Turning then to the second phase, the phase of science consciously applied, it is in general an illusion to suppose that it consisted in any utilization of the latest theory. The literature of chemical manufacturing makes frequent reference to the guidance offered by chemical theory to those well versed in it. But the theory in question was the theory of affinities, not the theory of combustion, and the theory of affinities was devoid of abstract interest. It did not in fact amount to much more than a tentative classification of substances according to their relative activity. If one read only the memoirs on the soda industry, one would suppose that the great chemist of the century was Richard Kirwan.¹ Lavoisier is never even mentioned. Leblanc and his kind needed knowledge of the chemical properties of substances. They did not need to understand combustion. Similarly in the dyeing industry, the line from Dufay to Berthollet does not pass through Lavoisier. In basic chemistry, on the other hand, neither does the line from Lavoisier to Gay-Lussac and Dalton pass through Berthollet. It was, indeed, the attempt to make it do so, misguided by the current fashion in scientific explanation, which led Berthollet to expand an excellent memoir on mass action into that curious and shapeless book, *Essai de statique chimique*,² quite unworthy of his real abilities, in which he tried to fix the elements of chemical science in the circumstances instead of the materials of reactions.

The true mode in which science was actively applied to industry has been obscured by the tendency of modern historical writers to suppose that the framing of basic theories is the main business of science, and that if science was related to industry, it must have been through the medium of advancing theory. They have failed to pay attention to the language of their texts, where the relationship of science to industry is not only clear but so clearly a corollary of the eighteenth-century

¹ See, for example, the translation of Kirwan's procedure for the analysis of soda, *Annales de chimie*, vol. 18 (1793), pp. 163-220.

² (2 vols.; Paris, 1803); Berthollet's adumbration of the law of mass action, 'Recherches sur les lois de l'affinité', was published in the *Mémoires de la Classe des Sciences . . . de l'Institut*, vol. III, pp. 1-96.

conception of scientific explanation that it required no explicit formulation. For the eighteenth-century scientists do not write of the application of theory. What they say is that science illuminates the arts, that it enlightens the artisans, and that this process honours the century, and in holding this language they are simply considering industry in the light of that pervasive notion of the function of scientific explanation which (after Locke) is to be found in Condillac and Condorcet, in Jussieu and Carnot, in Lavoisier, Lamarck, and Laplace, in the physiocrats, the *idéologues* and the first *École normale*. In this light, science itself is positive knowledge, of course. Its function in the world is essentially an educational one, however, and its mode of procedure is analytical. First, it seeks to discern the essential elements of a complex subject. These once found, it ranges and classifies them according to the logical connexions which subsist underneath all the welter of phenomena. Next, it establishes a systematic nomenclature designed to fix the thing in the name, fasten the idea to its object, and cement the memory to nature. In this fashion, the human understanding will be led descriptively towards a rational command over every department of nature by following its inherent order. Scientific explanation, then, consists in resolving a subject into its elements in the objective world in order that it may be reassembled in the mind according to the principles of the associationist psychology. The inspiration was algebra. But the model was botany.

It is consistent, therefore, that when scientists turned to industry, it was to describe the trades, to study the processes, and to classify the principles. In this taxonomic fashion science was indeed applied to industry, and very widely. What were those enormous ventures, the Academy's *Description des arts et métiers*, the *Encyclopédie* itself, the *Encyclopédie méthodique*, if not attempts to lift the arts and trades out of the slough of ignorant tradition and by rational description and classification to find them their rightful place within the great unity of human knowledge? The eighteenth-century application of science to industry, then, was little more and nothing less than the attempt to develop a natural history of industry. In this sense, the scientific development of an industry is measured, not by the

degree to which new theory is used to change it, but by the extent to which science can explain it theoretically. 'We are frequently able', writes Berthollet (the quotation is from the first translation of his *Éléments de l'art de la teinture*), 'to explain the circumstances of an operation which we owe entirely to a blind practice, improved by the trials of many ages; we separate from it everything superfluous; we simplify what is complicated; and we employ analogy in transferring to one process what has been found useful in another. But there are still a great number of facts which we cannot explain, and which elude all theory: we must then content ourselves with detailing the processes of the art; not attempting idle explanations, but waiting till experience throws greater light upon the subject.'¹ Similarly, the metal industries were not at first much changed by the development of the science of metallurgy; they simply began to be understood. But that processes will be altered for the better if their principles are understood, that artisans will improve their manipulations if they know the reasons for them, are simply illustrations of the eighteenth-century faith in progress through classification and industrial examples of the eighteenth-century belief in scientific explanation as a kind of cosmic education.

Accordingly, the revolutionary manufacture of saltpetre consisted essentially in subjecting the French people to mass instruction in a simple technical process. What Monge, Fourcroy, Guyton, Berthollet, and the others did when they were brought into this enterprise, was to give popular courses.² The military crisis of 1793-4 created the greatest practical incentive ever experienced for the application of science to production in general. The Committee of Public Safety urged it forward with all its fearful authority. And inevitably the project assumed an educational form; science was mobilized in defence of the Republic, and at great speed scientists produced a series of textbooks, instructing practitioners, not so much in

¹ *Elements of the Art of Dyeing* (Edinburgh, 1792), p. 17.

² On the *cours révolutionnaires*, see C. Richard, *Le comité de salut public et les fabrications de guerre sous la Terreur* (Paris, 1922), pp. 469-86; *Procès-Verbaux du comité d'instruction publique* (Paris, 1901), vol. IV, pp. xxi-xxviii; the text of the courses is at the *Archives nationales*, AD VI, 79, pièce 69.

new methods, as in the best methods.¹ Finally (not to labour illustrations), it may perhaps be consistent with the nature of the mathematical achievements of Monge in descriptive geometry that he should have been the one important mathematician who consistently expressed the utilitarian valuation even of mathematics and that he should also have been the moving spirit in the foundation of the *École polytechnique*, that portentous institution.²

Nor was the faith in science as the educator of industry simply abstract, for their relations can be brought down to a question of what people actually did. Chaptal's testimony may again be quoted:

Mais du moment que la chimie est devenue une science positive; surtout lorsqu'on a vu des chimistes à la tête des plus grandes entreprises, et faire prospérer dans leurs mains plusieurs genres d'industrie, le mur de séparation est tombé, la porte des ateliers leur a été ouverte, on a invoqué leurs lumières; la science et la pratique, se sont éclairées réciproquement, et l'on a marché à grands pas vers la perfection.³

Chaptal dates this change from the Revolution when the government 'pressé par le besoin, a successivement tiré plusieurs savans de leur cabinet pour les placer dans les ateliers, et la plupart y ont fait des prodiges en très-peu de temps'. And there

¹ For the present purpose, it would burden this note unnecessarily to cite full titles. Suffice it to say that treatises by leading scientists were published on the following trades or processes: iron-working; small-arms; the casting and boring of artillery pieces; the incineration of plants (for potash); the soda industry; the soap industry; tanning; the separation of copper from bell metal; etc. Most of these treatises are gathered together in the *Archives nationales*, AD VIII, 40. But relatively little was attempted in the way of innovation. Perhaps the most interesting exception was the establishment in the year II of the *Atelier de perfectionnement*, an embryonic technological development laboratory, under the direction of Vandermonde, where experiments were made looking towards standardized and rationalized production, including the introduction, where appropriate, of interchangeable parts. But little came of this hopeful venture, which was eventually absorbed in the *Conservatoire des arts et métiers*. For documents concerning this project, see *Archives nationales*, F¹² 233, 234, 1310; F¹³ 288.

² Besides the excellent study of Monge's work by René Taton, *L'oeuvre scientifique de Monge* (Paris, 1951), there is also available a rather inferior recent biography, Paul V. Aubry, *Monge, le savant ami de Napoléon Bonaparte* (Paris, 1954). For the caution with which this must be used, see my review in *Scripta Mathematica*, vol. 22 (1956), pp. 245-6.

³ *De l'industrie française* (2 vols.; Paris, 1819), vol. II, pp. 38-9.

is ample confirming evidence that a new generation of scientifically instructed entrepreneurs and managers came into control of industrial operations during the Revolutionary period, and that the Revolution saw the culmination and the end of the very real hostility between scientists and artisans.¹ As a result, it was no longer necessary to complain constantly of the obstruction of rational procedures by the ignorance and traditionalism of the ordinary artisan, always singled out as the greatest barrier to progress. Popular superstition was the *bête noire* of the rational writer, and whether he looked to religion or technology, he found it flourishing in ignorance and secrecy. To publicize processes, therefore, to get them out in the light of day, must be the business of science, and the importance attached to this is evident in the condition of publicity attaching to the prize programme of the *Académie des sciences* and the grants of the *Bureau de consultation*. That such was also the policy of the English Society of Arts is an indication that the apparent differences in the relations of science and industry in France and Britain were more verbal than real.² There is no contesting the reality of British industrial leadership in the eighteenth century, but it is conceivable that the industrial interests of British scientists were more a result than a cause of this pre-eminence. If they did not relate them to some general outlook, this circumstance may possibly be taken as another illustration of the historical law binding the two great western societies which prescribes that the French should formulate what the British only do.³ In any case, it is, perhaps, not overly fanciful

¹ See, for example, C. A. Costaz, *et al.*, 'Rapport au 1^{er} Consul', *Bulletin de la Société d'Encouragement*, vol. 1 (1802), pp. 45-8.

² See Derek Hudson and K. W. Luckhurst, *The Royal Society of Arts* (London, 1954).

³ Far from regarding Dr Schofield's accompanying paper, 'The Industrial Orientation of Science in the Lunar Society of Birmingham', as calling for conclusions different from those which I base on a study of French developments, I think his material calls for a similar conclusion and could be better taken as a confirmation than a refutation of my point. The difference lies no doubt in what we mean by science. I mean abstract understanding of nature. Dr Schofield seems to mean technical activity. What is the relationship of any of the scientific work alluded to by Dr Schofield to basic theory? Indirect, at best – in fact this work was precisely the sort which led French writers sympathetic to the utilitarian or Jacobin tradition to describe the Revolution as favourable to science. But perhaps this question of interpretation had better be left to the reader.

to summarize the problem by contrasting the enterprising, bold manufacturer of the nineteenth century, the engineer, the industrialist, in whatever country, to the Gothic master-craftsman of olden times, protecting his secrets and his mysteries, bending over his cauldron and stirring some traditional receipt, some confidential brew. The application of science to industry takes on real meaning, then, if it is seen, not naïvely as the alteration of old practices by theoretical concepts, but rather as an intellectual process and a chapter in the history of the Enlightenment. 'Au tableau des sciences,' writes Condorcet of his century, 'doit s'unir celui des arts qui, s'appuyant sur elles, ont pris une marche plus sûre, et ont brisé les chaines où la routine les avait jusqu'alors retenus.'¹

¹ *Esquisse d'un tableau historique des progrès de l'esprit humain* (Paris, 1795), p. 296.

5 *The Industrial Orientation of Science in the Lunar Society of Birmingham*

ROBERT E. SCHOFIELD

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The historical relationship of science with technology is a complicated one and conclusions as to the extent of that relationship vary depending on the country and the perspective in which the problem is studied. Dr C. C. Gillispie has presented us with his conclusions based on a study of France in the revolutionary period and the early years of the empire.¹ He suggests that the relationship for this period in France is, at best, an indirect one.

The same type of argument can be – and indeed has been – made for Great Britain for approximately the same period. It is suggested, however, that were the same conclusion drawn for this period in Great Britain a mistake would be made. The social patterns of scientists in late eighteenth-century England were quite different from those of France. Dr Gillispie refers to the ‘mutual stimulation of science, commercial enterprise, and Puritan influence . . . [characteristic] of British social, intellectual, and scientific history from the seventeenth through the nineteenth centuries.’² He also points out the very evident connexion between the consequent utilitarianism and the relative poverty of British achievements in abstract science during the years of greatest dominance of this stimulation. Few would deny this latter observation, but that is not really the point here. For if one studies the activities of the scientists responding to this stimulation, one must conclude

¹ C. C. Gillispie, ‘The National History of Industry’, *Isis*, vol. 48 (1957), pp. 398–407. See above, chap. 4.

² Gillispie, *op. cit.*, p. 403.

that science and industry were, by that very fact, more closely and more deliberately related in Great Britain than in France.

One cannot approach the problem of science and technology in late eighteenth-century England or Scotland by referring to the technological activities of the Royal Society, for the Royal Society was in its doldrums during this period. Nor can even the activities of the Society Instituted at London for the Promotion of Arts, Manufactures, and Commerce (i.e., the Society of Arts, later the Royal Society of Arts) be used as a standard. However influential the Society of Arts may have been in encouraging (and spreading information about) technological improvements, it cannot seriously be maintained that there was much of a scientific nature in its work. For many an eighteenth-century English scientist, membership in the Royal Society was a matter only of social prestige, while eighteenth-century English manufacturers were members of the Society of Arts on the off-chance that something useful might almost accidentally result from its endeavours. These two groups, the scientists and the manufacturers, combined however in one type of organization – the provincial scientific societies – a study of which enables us to draw rather different conclusions for England than Dr Gillispie could draw for France.

One of the most characteristic developments of eighteenth-century English science was the proliferation of provincial scientific societies. There was a Manchester Literary and Philosophical Society, a Derby Philosophical Society, a Literary and Philosophic Society of Newcastle-on-Tyne, and philosophic clubs in Liverpool, Bristol, Leeds, and many other places. It may not be significant that the most active and successful of these societies were founded in manufacturing centres in the North and Midlands of England. These were, after all, the growing population centres and might be assumed to be more likely places for this type of activity. Much more to the point, at any rate, is the fact that prominent manufacturers were themselves members of such societies – e.g., the Strutts of Derby, the Lloyds, Brandts, and Phillips of Manchester, and Thomas Bentley of Liverpool were all members of such

societies.¹ Of all the provincial scientific societies, the one which most exemplifies the joining of interests of manufacturer and scientist is the Lunar Society of Birmingham and of all such societies this is perhaps the best known – in spite of the fact that less exact information has been published about the Lunar Society than about many of the other societies of the period. This is one of the reasons why an extensive study of this society has been undertaken.² The remainder of this paper is a report of some of the results of that study.

The Lunar Society left no formal records; the only published contemporary references to the society were those made by Joseph Priestley – chiefly in the dedication of his *Experiments on the Generation of Air from Water* (1793) and in his *Memoirs*, published posthumously in 1806. In these publications Priestley gives a list of some of the members, describes the origin of the name of the Society, and tells what the Society did not do. He fails to give much indication of what the Society did do. Not until Samuel Smiles investigated the papers of Matthew Boulton and James Watt did we get any serious recognition of the significance of the Lunar Society and Smiles's account³ contains too many pure speculations for comfort. Nevertheless, directly or indirectly, Smiles's work has provided the source of all subsequent accounts of the Lunar Society, and his speculations were only the beginnings of what has developed into a Lunar Society legend of extraordinary proportions.

An investigation of original sources was needed to interrupt the perpetuation of this legend. With this in mind, a study was begun of the letters, manuscripts, and publications

¹ See Eric Robinson, 'The Derby Philosophical Society', *Annals of Science*, vol. 9 (1953), pp. 359–67. R. Angus Smith, *A Centenary of Science in Manchester* (London, Taylor and Francis, 1883), pp. 32–3; and R. B. (Richard Bentley), *Thomas Bentley, 1730–80, of Liverpool, Etruria and London* (Guildford, Richard Bentley, 1927).

² My research on the Lunar Society, a small part of the results of which are presented in this paper, was done chiefly from MSS in the collections of the Wedgwood Museum, The Royal Society, The Royal Society of Arts, The Assay Office, and the Reference Library of Birmingham, and the Darwin Museum, Down House, Downe, Kent. I take this opportunity to acknowledge the assistance of the librarians and the curators of these collections.

³ Published in his *Lives of Boulton and Watt* (London, John Murray, 1865).

of the persons most commonly assumed to have been members of the Society. It was hoped that we might establish a more accurate membership list than had previously existed and that something reasonably definite could be learned about the activities of the Society. Both of these hopes have, to some extent, been justified. We can now give a credible list of those persons whose associations were of a sort entitling them to be named members of the Society. That list includes:

MATTHEW BOULTON (1728–1809), manufacturer of metal products and later partner with James Watt in the production of steam engines.

ERASMUS DARWIN (1731–1802), physician, poet, and dabbler in scientific speculation of all sorts; grandfather of Charles Robert Darwin and Francis Galton.

THOMAS DAY (1748–89), gentleman eccentric, philanthropist, interested chiefly in politics and metaphysics.

RICHARD LOVELL EDGEWORTH (1744–1817), Irish landowner, inventor of miscellaneous mechanical contrivances, interested in agriculture and education, father of Maria Edgeworth.

SAMUEL GALTON, JR. (1753–1832), Quaker gun-manufacturer, dilettante in many sciences – particularly ornithology and optics, grandfather of Francis Galton.

ROBERT AUGUSTUS JOHNSON (?–1799), Anglican clergyman, FRS, but otherwise unknown.

JAMES KEIR (1735–1820), chemist, geologist, chemical manufacturer and mine operator.

JOSEPH PRIESTLEY (1733–1804), Unitarian clergyman, experimenter in electricity and chemistry.

WILLIAM SMALL (1734–75), physician, metallurgist, one-time Professor of Natural Philosophy at the College of William and Mary, teacher of Thomas Jefferson.

JONATHAN STOKES (1755–1831), physician, botanist, chemist.

JAMES WATT (1736–1819), inventor, engineer, chemist.

JOSIAH WEDGWOOD (1730–95), potter and chemist.

JOHN WHITEHURST (1713–88), instrument maker, geologist.

WILLIAM WITHERING (1741-99), physician, botanist, chemist.¹

This is an extraordinary collection of people. Only Robert Augustus Johnson left no record of his activities; all the rest made significant contributions to the culture of their period and many are still classed as among the more important individuals of eighteenth-century England. It is, of course, that fact that encouraged the interest in the Lunar Society. With some justice, it was felt that men of this calibre do not regularly associate with one another over trivialities.

We can arrive at some idea of the work of the Society by comparing the letters and published works of these members. It is assumed that the interest of more than two of these individuals in any subject at the same time may reasonably be claimed as an interest of the Society. Acting on that assumption, one arrives at two major conclusions. First: the meetings of the Society were comparatively unimportant. Members living in reasonably close proximity associated with one another almost daily; those living, or temporarily occupied, outside the Birmingham area were kept in constant touch with the other members through continual correspondence – usually averaging at least one letter per week from some one of the Birmingham residents. The meetings gave rise to the name of the Society, but otherwise they seem important only as a social cement for keeping the group together. Information was exchanged between members outside meetings as freely and more frequently than inside them. Second: the kind of problems most commonly considered demonstrates so clearly an industrial orientation of scientific interest that it is not unreasonable to claim the Lunar Society as an informal technological research organization.

The nucleus of that organization was created by the meeting of Boulton and Darwin sometime before 1760. These two, one living in Birmingham and the other in Lichfield, less than fifteen miles from Birmingham, were brought together by a common love of science and a shared admiration for Benjamin Franklin,

¹ The justification of this choice of members will be contained in a more extensive treatment of the Lunar Society now being prepared.

to whom they were introduced by a letter from John Michell. Franklin provided an early link between various members-to-be of the Lunar Society; he was already, or was to become, the friend of Priestley, Day, Small, and Whitehurst, and a patient of Withering. Franklin's interest in electricity encouraged Boulton and Darwin in their experiments on electricity. Darwin's first published scientific work was a paper, 'Remarks on the Opinion of Henry Eeles, Esq. . . . Concerning the Ascent of Vapour',¹ which discusses the behaviour of 'electric matter' and includes a practical query as to the possible substitution of that 'matter' for steam in steam engines. Boulton had more direct practical interests in electricity. He began the manufacture (for sale) of a small number of electrical machines and later added permanent magnets to his stock-in-trade. Priestley first entered the orbit of Lunar Society members when he referred to Darwin's work in his *History of Electricity*.² At about the same time, Wedgwood met Priestley and began to speculate about a possible application of Priestley's electrical experiments to the problem of gilding pottery.³ From this time on, Priestley was to be in contact with Lunar Society members, although he did not become a member himself until late in 1780 or early in 1781.

An interest in the improvement of transportation is characteristic of the Industrial Revolution and could be found in most enlightened Englishmen of the eighteenth century. In the Lunar Society, this interest took many forms. Boulton, Darwin, Small, Watt, and Wedgwood concerned themselves in turnpike trusts and canal projects, Edgeworth considered the possibility of improving road design and is credited by some with having anticipated Macadam.⁴ A common interest in the improvement of carriage design brought Edgeworth together with Boulton and Darwin. Boulton's work seems to have been

¹ *Philosophical Transactions*, vol. L (1757), p. 240.

² Joseph Priestley, *History and Present State of Electricity* (London, J. Dodsley, J. Johnson, B. Davenport, and T. Cadell, 1767), pp. 215, 264.

³ E. Metayard, *Life of Josiah Wedgwood* (London, Hurst and Blackett, 1865), vol. I, p. 388.

⁴ See, for example, Henry Law and D. K. Clarke, *The Construction of Roads and Streets*, 6th ed., revised with additional chapters by A. J. Wallis-Taylor (London, Crosby, Lockwood and Son, Ltd, 1901).

confined to empirical studies, but Darwin's Commonplace Book shows an application of theoretical considerations to the design of wheel mountings, while Edgeworth approached the problem as an experimental study in basic mechanics.¹ Both Darwin and Edgeworth communicated their designs to the Society of Arts² and both considered the idea of manufacturing carriages of their improved design – a project in which they interested Wedgwood – though nothing came of their plan.

Even before James Watt became part of the Lunar circle, members had been interested in steam engines. We have already referred to Darwin's paper of 1757 in which steam engines were mentioned; his correspondence with Boulton shows a continued interest in steam engines down to the date when Watt joined the group (about 1767). Edgeworth hoped to drive carriages and boats by steam. In 1768 he constructed a steam-engine model of his own design and submitted it to the Society of Arts.³ Several years before he met Watt, Boulton had consulted Franklin about steam-engine design. He also constructed a model of an engine which was sent to Franklin for suggestions. Details of Edgeworth's and Boulton's work are unknown, but it is probable that both approached the problem empirically. The introduction of Watt's work naturally changed the situation. Darwin and Edgeworth turned their attention to other problems, Boulton enlisted the help of William Small and attention was given to making Watt's scheme operable on a large scale. One important consideration in this respect was the metallurgical problem involved – especially in the design of piston seals. Small began an intensive study of metallurgy, during which he obtained the assistance of Whitehurst and read all the current texts on metallurgy he could find – even-

¹ MS Commonplace Book, Darwin Museum, Down House, Downe, Kent; R. L. Edgeworth, 'On Wheel Carriages', *Transactions of the Royal Irish Academy*, vol. II (1788), pp. 73–80.

² Letters of Erasmus Darwin and Richard Lovell Edgeworth to Ralph Templeman, 8 March 1766 and 15 May 1766, respectively (Archives, Royal Society of Arts).

³ Minutes of Committees 1768–9: Mechanics, 5 January 1769 (Archives, Royal Society of Arts). The model and description have since been lost. Edgeworth was not aware of Watt's work, since both he and Watt at that time were in contact with the Lunar Society only through correspondence with members of the society, who naturally did not divulge one another's secrets.

tually condemning all of them as inadequate. Boulton, as a manufacturer of metal products, had long been interested in metallurgy on a wider scale. He experimented with different metals and ores, worked with Keir in devising new alloys and new working technics, and had Withering translate the mineralogical treatise of Bergmann for his use.¹

While we must agree that the discovery of the latent heat of vaporization was not a significant factor in Watt's invention of the separate condenser,² we must not be carried to the extreme of deciding that Watt's work was simply good empiricism. The best example of empiricism well applied to a study of steam engines is the work of John Smeaton.³ One need only compare Smeaton's work with Watt's to recognize a decided difference in approach to the problem – a difference most adequately described by insisting that Watt was the more scientific of the two. He demonstrated his early recognition of the importance of more theoretical considerations in improving the design of steam engines by his assistance of Joseph Black in the investigation of the properties of heat and steam. He continued these investigations in Birmingham and obtained the assistance of Boulton, Wedgwood, Whitehurst, and Withering. Boulton and Wedgwood had other reasons to be interested in the question of heat. Boulton had manufactured thermometers for sale since early in the 1760s, while Wedgwood's manufacturing processes involved the use of high temperatures. Erasmus Darwin's son, Charles, sent both of them copies of his notes of Black's lectures. Wedgwood subsequently developed a ceramic pyrometer for use in measuring high temperatures and then produced the pyrometer for sale. Whitehurst performed experiments to determine whether heat had weight,⁴ and Watt had Withering perform experiments on 'heating iron red-hot by hammering'.⁵ Priestley was involved

¹ Torbern Bergmann, *Outlines of Mineralogy*, trans. W. Withering (Birmingham, for T. Cadell and G. Robinson, 1783).

² See, for example, D. Fleming, 'Latent Heat and the Invention of the Watt Engine', *Isis*, vol. 43 (1952), pp. 3-5.

³ See Farey, *Treatise on the Steam Engine* (London, 1827).

⁴ John Whitehurst, 'Experiments on Ignited Substances', *Philosophical Transactions*, vol. LXVI (1776), pp. 575-7.

⁵ Letter from Watt to Black, 9 March 1780; quoted by J. P. Muirhead, *Origins*

in steam-engine research first when he inspected a steam-turbine design of Kempelen at the request of Watt and again when, at the request of Boulton, he investigated the possibilities of substituting the chemical reactions of gases for steam condensation as a source of energy for engines. He also investigated the properties of steam; his experiments led Watt to make suggestions about the composition of water.

Whitehurst's interest in geology gave him a connexion with industrial problems of Wedgwood and Boulton. Boulton's interest in geology stemmed from his financial operations in Cornwall, where he eventually established a research assay office to investigate the properties of the various ores found there. He and Whitehurst went on mineralogical expeditions together. Wedgwood supplied Whitehurst with specimens and descriptions of canal diggings, while Whitehurst sent Wedgwood samples of stone and clay which might be of use in ceramic experiments. Whitehurst was, for example, one of the sources from whom Wedgwood obtained the barium carbonate, or witherite, he used in his jasper-ware. The name 'witherite' was given to this substance because William Withering was the first person to perform a significant analysis of it.¹

Priestley, Watt, and Keir were also involved in Wedgwood's ceramic investigations. Wedgwood called upon Priestley for experiments on ceramic materials, and sent him samples to experiment on. Wedgwood read Priestley's chemical works for insight into the theoretical interpretations of chemical-ceramic reactions. He reciprocated Priestley's services by supplying him with gifts of chemical apparatus. Similar apparatus was later sold to Boulton, Watt, Keir, Withering, and Johnson. Watt became involved in pottery problems because he owned part of a pottery works in Scotland and had assisted in directing its operation. Wedgwood obtained samples of clays from Watt and quoted Watt's interpretation of the behaviour of pipe-clay when subjected to high temperatures.²

and Progress of the Mechanical Inventions of James Watt (London, John Murray, 1854), vol. 2, p. 118.

¹ *Philosophical Transactions*, vol. LXXVII (1784), pp. 293-311.

² Wedgwood Commonplace Book: '... series extracted from experiment book: 21 June 1780' (Wedgwood Museum, Josiah Wedgwood and Sons, Ltd, Barlaston, Stoke-on-Trent).

Keir, who managed a glass-works for a time, shared Wedgwood's interest in annealing processes. Keir experimented on glazes for Wedgwood, and Wedgwood returned the favour by experimenting on the improvement of glass-making. Keir's glass-works involved him also in geological speculation, when he noticed that molten glass will crystallize if slowly cooled. He suggested that this observation might be extended to the case of basaltic crystals which could then be established as of volcanic origin.¹

Keir shared with Priestley, Watt, Wedgwood, and Withering an interest in chemistry, but Keir made a career of it. He deliberately prepared himself for his chemical career by the translation of the best practical chemical treatise of the period – the chemical dictionary of Macquer – and then went into a chemical manufacturing business which rivalled Boulton and Watt's Soho operation in size and in the variety of materials produced. His chemical works at Tipton made red and white lead, soap, acids, and alkali, the latter from the waste products of other processes. He had previously experimented with the manufacture of alkali from salt, as had James Watt, in company with Joseph Black and John Roebuck. Wedgwood and Darwin were great admirers of Keir's translation of Macquer.

Some of Keir's notes to the translation refer to a theory of dyeing. According to Thomas Henry: 'Mr Keir . . . appears to have been the first who suspected that the earth of alum [used as a mordant] was precipitated, and in this form attracted to the material.'² Other Lunar Society members also gave some consideration to the cloth industries. Erasmus Darwin attempted improvements on stocking-frame design, Withering consulted friends about dyeing theory, while Watt invented steam drying-machinery and returned from a visit to Berthollet with the news of the bleaching properties of chlorine, a discovery which he was one of the first to introduce into England.

Priestley constantly emphasized the practical potentialities

¹ *Philosophical Transactions*, vol. LXVI (1776), pp. 530–42. This was twenty-two years earlier than the similar suggestion of Sir James Hall.

² Quoted by R. Angus Smith, *A Centenary of Science in Manchester* (London, Taylor and Francis, 1883), p. 112.

of his chemical discoveries. His first public chemical announcement dealt with the possible use of 'fixed air' or carbon dioxide as an anti-scorbutic; he also commented, at the time he announced his discovery of oxygen, on the possible medical application of this new 'air'. The investigation of the medicinal use of gases was carried out later at the Pneumatic Medical Institution of Thomas Beddoes, the son-in-law of Richard Lovell Edgeworth. Beddoes's work was partially supported by contributions from Wedgwood, Boulton, and Watt. Watt joined Beddoes in experimenting on this problem and Boulton and Watt manufactured and sold apparatus for the use of Beddoes's patients.

The Industrial Revolution also included an agricultural phase, and we find Lunar Society personnel participating in this 'agricultural revolution'. Priestley's discovery of the way plants make use of carbon dioxide involved him in a correspondence with Arthur Young about agriculture. William Withering's first published non-medical, scientific paper concerned the production of a chemical fertilizer,¹ and Erasmus Darwin wrote a book, the *Phytologia* (1800), about agriculture. Boulton, Wedgwood, Withering, Edgeworth, and even Day concerned themselves with improvements in agricultural operations.

This paper cites only briefly some of the attempts of the Lunar Society members to apply scientific knowledge and scientific processes to the technical problems of the Industrial Revolution. It is true that many of these attempts may also be described as the application of scientists and the exploitation of science,² but if the scientists were applied, it was their own idea, and it is not always easy to distinguish, even today, between the application and the exploitation of science. Then, as today, science was frequently used to explain a manufacturing process developed independently of the science, but these explanations were used by Lunar Society manufacturers in an attempt to improve those processes. Furthermore, the manufacturers

¹ 'Experiments upon the different kinds of Marl found in Staffordshire', *Philosophical Transactions*, vol. LXIII (1773), p. 161.

² See Gillispie, *op. cit.*, p. 404, where these terms are suggested as a more adequate description of French practice than is 'applied science'.

themselves contributed, or attempted to contribute, to the development of the 'pure' science of their day. If their contributions in this line were rather minor, so also were the contributions of the majority of their 'purer-scientist' colleagues. If, like Boulton, a few were rather more inclined to make use of the scientific work of their fellow society members than to contribute by their own studies, still they were prepared to make financial grants to enable the continuation of research in pure science, if only in the hope that something useful might develop. In what way does this significantly differ from the motives behind the great industrial research laboratories of today where study of pure as well as applied science is supported?

The examples of Lunar Society activities cited here are only a few of those which touch on science and industry. These and the many others should, of course, be studied more intensively, but even so cursory an account should adequately have demonstrated the validity of our claim that the Lunar Society represented an eighteenth-century technological research organization. It would be hard to find a single activity, of either the science or the technology of the eighteenth century, in which more than one Lunar Society member cannot be found to have been involved – usually with an attempt to turn his knowledge to practical advantage. No consideration of the relationship between science and technology during the early years of the Industrial Revolution can afford to ignore the activities of the Lunar Society. It seems reasonable to suggest that a study of the activities of other provincial scientific societies might prove equally rewarding.

6 Vitriol in the Industrial Revolution

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In 1843 Justus Liebig made the following generalization:

We may fairly judge of the commercial prosperity of a country from the amount of sulphuric acid (oil of vitriol) it consumes.¹

By 1843, oil of vitriol had been manufactured in Great Britain for over a century, during which time it brought about more than one revolution in social technology, yet even today it is impossible to find an extended account of the first century of this fundamental industrial chemical, which of all *materia chemica* had the greatest effect on the course of late eighteenth- and early nineteenth-century economic history.

Oil of vitriol was first used as an apothecaries' nostrum, and it is for use as such that we have our first account of its manufacture in England. In 1733 Joshua Ward (1685-1761) returned to England from France whence he had gone to evade justice in connexion with a parliamentary offence. Three years later, in partnership with John White, he set up a vitriol works at Twickenham where he proceeded to make vitriol, or sulphuric acid, in large glass vessels, or as it was then described, *per campanam*. Ward is variously described as a quack doctor, vendor of analeptic pills, and inventor of the noted white drops. In 1740 Ward and White removed from Twickenham to Richmond.² The important point about Ward and White's establishment is that it reduced the price of oil of vitriol or sulphuric acid from between 1s. 6d. and 2s. 6d. per oz to between 1s. 6d. and 2s. 6d. per lb, thus paving the way for it to

¹ *Familiar Letters on Chemistry*, p. 31.

² H. W. Dickinson, *Newcomen Society Transactions*, vol. XVIII (1937), p. 43.

become the particular substance which contributed a major part to the non-mechanical aspect of the Industrial Revolution. It is, therefore, to industrial rather than to pharmaceutical application that we must turn for the impetus that stimulated the subsequent rapid expansion in its production.

Throughout the literature of the early Industrial Revolution we find sporadic reference to the use of sulphuric acid; more often, however, all that is said is that the applications of sulphuric acid are so well known that they do not need enumeration. Of particular significance in view of the *locus* of the second vitriol works in England was its use by brass-founders, button-makers, japanners, gilders, refiners, tinplate-makers, tanners, hatters, and paper-makers, most of whom used it for metal-cleaning and pickling. That the second set of persons to take up the manufacture of sulphuric acid in England should be consulting chemists, refiners, and recoverers of silver and gold, and the site of their works Birmingham, need occasion no surprise.

The manufacturers were John Roebuck (1718–94) and Samuel Garbett (1717–1805), the former a doctor of medicine trained at Edinburgh and Leyden, the latter a Birmingham business-man. In 1746 they began making sulphuric acid as an adjunct to their refinery at Steelhouse Lane, Birmingham – a pivotal event in eighteenth-century economic history.

Sulphuric acid is a highly corrosive liquid and, considering the state of English roads at the middle of the eighteenth century, it must have been a load only transportable with considerable risk. The setting up of Roebuck and Garbett's factory brought the actual production of sulphuric acid into a district where it would be used in quantity. Furthermore, profiting by an observation of the alchemist Glauber that lead is not attacked by the corrosive liquid, they abandoned the glass vessels used by Ward and built their plant wholly of lead. This removed limits set by size and fragility of glass and made possible a further reduction in the cost of the acid.

In 1749 Roebuck and Garbett established a second vitriol works, this time at Prestonpans, on the Firth of Forth a few miles east of Edinburgh, in a district where manufacture of salt, pottery, and glass was already established. The reason

behind the great change in locality is not known definitely, but our thesis is that Roebuck, who derived his interest in chemistry from Professor Plummer of Edinburgh, under whom he had studied, saw in Scotland a market for his sulphuric acid not offered by England.

Over a long period, during the earlier part of the eighteenth century, the Board of Trustees for Fisheries, Manufactures, and Improvements in Scotland did its utmost to effect improvement in the finishing of Scottish linen by subsidizing the laying down of bleachfields and by fostering research into new methods of bleaching. According to Andrew Brown's *History of Glasgow*, the bleachfields established in Scotland under the patronage of the Board of Trustees were supplied with what sulphuric acid they needed in the first instance (i.e. before 1750) by importation from England or Holland at a price of 1s. 4d. per lb.¹ This indicates that before 1750 some sulphuric acid was substituted for the acid formerly used in the primitive bleaching industry, viz., the acid in sour milk. The process of bleaching was then a simple process consisting of boiling alternately with ashes (alkali) and a *sour* (sour milk or sulphuric acid) with intervening exposure to the sun.

We know also that Roebuck himself had experimented with sulphuric acid, since his biographer Jardine says:

It is well known to several of Dr Roebuck's chemical friends that he had tried it, found it effective, and had frequently recommended it to bleachers.²

The first official notice of the use of sulphuric acid was the publication in 1756 at the instigation of the Board of Trustees of a memoir, *The Art of Bleaching*, by Francis Home (1719–1813), professor of *materia medica* at Edinburgh. In his memoir Home describes the researches for which the Trustees awarded him a premium on 15 April 1756. According to Home,

the milk takes five days to perform its task, but the vitriol sours do it in as many hours, nay, perhaps in as many minutes.

¹ Vol. II (1795), p. 250.

² *Transactions of the Royal Society Edinburgh*, vol. IV (1796), p. 65.

The increased *tempo* effected by substituting sulphuric acid for sour milk was early appreciated by the finishing trades. Other branches of the finishing trades also made use of the cheap acid, the dyers for dissolving indigo, calico-printers for making *sours* and in the preparation of citric acid, of which they used a great deal.

Roebuck and Garbett's sales throughout Great Britain are not now known, but we can follow the rise of their export trade from Customs House returns for Prestonpans preserved in the General Register House, Edinburgh. The first recorded export is:

14 March 1750-1: in the Huckster of Hamburgh, for Hamburgh, twenty large bottles containing 1990 pounds wt. of Oyle of Vitriol. British manufacture to be exported duty free.

Two months later:

14 May 1751: In the Buccleugh of Fisherrow for Dunkirk, sixteen large bottles containing 1665 pounds wt. of Oyl of Vitriol. Three large bottles containing 203 pounds wt. of Aqua Fortis.

Two years elapse before another export is recorded, but the quantity has greatly increased:

16 February 1753: in the Hope of Leith for Rotterdam, 100 bottles containing 11,179 pounds wt. oil of vitriol.

Cargoes follow to Bremen, Copenhagen, and Campvere, the average cargo being by 1756 about 25,000 lb weight contained in two hundred bottles:

Garbett's enterprising spirit and able management rewarded them well, providing them with considerable capital for their future projects. Articles were drawn up between the two, for carrying on the partnership at Prestonpans and Birmingham over a period of forty years.¹

Following Roebuck and Garbett's two foundations, a third vitriol works was founded at Bradford. Although without

¹ P. S. Bebbington, *Samuel Garbett, 1717-1803* (Thesis, Birmingham University).

definite records before 1792, it is probable that Benjamin Rowson founded a works there in 1750. Of the founder personnel we know nothing, but it is said to have a continuous history and to be represented at the present day by the Leather Chemical Co. Ltd.

The amount of sulphuric acid made by these works was considerable, and its cheapness a direct impetus to extended application. The transition from ashes and sour milk as bleaching agents can be followed in orders placed with London dry-salters by Messrs John and Nathaniel Philips and Co., tape-manufacturers of Tean, Staffs. In 1753-4 they ordered weed and wood-ashes, 'a cask of Danzig ashes to look at', alum, tartar, etc., while a decade later (1765-9) their orders were much more comprehensive, including, in addition to a big variety of ashes, sulphuric acid, probably bought from Roebuck and Garbett in Birmingham.¹

For more than twenty years Roebuck and Garbett relied upon secrecy to protect them from rivals who wished to ascertain the details of their process so that they might exploit it for their own benefit. During those twenty years, however, Roebuck and Garbett were the unwitting suppliers of information to industrial rivals who bribed their workmen and offered jobs to dismissed or absconding workmen from both Birmingham and Prestonpans.

This was the origin of a sulphuric acid works at Bridgnorth on the Severn, established in 1756 by a Mr Rhodes, who was persuaded to do so by Sam Falconbridge, an employee of Roebuck's.² Some time later, the same workman appears to have moved to Mr Skey (1726-1800), a dry-salter of Bewdley, who had just begun to make vitriol at Dowles in chambers erected under the supervision of a workman from Prestonpans whom he had met by accident.³

In consequence of these developments and the periodic appearance of suspicious characters in the neighbourhood of Prestonpans, Roebuck and Garbett, on 9 August 1771, regis-

¹ A. P. Wadsworth and J. de L. Mann, *The Cotton Trade and Industrial Lancashire, 1600-1780* (1931), p. 296.

² Signet Library, Edinburgh, *Session Papers*, F. 166: 18.

³ S. Parkes, *Chemical Essays*, vol. II, (1815), p. 399.

tered a patent in fulfilment of a promise in letters-patent granted by the King to work their process in Scotland for fourteen years, their particular invention being the use of lead vessels for all operations. The parchment recording the patent and signed by Roebuck is in the General Register House, Edinburgh.¹

Roebuck tells how persons 'who kept their names and their business a profound secret' were apt to appear. One of these was Neil Macbrayne, who came in disguise to Prestonpans in June and July 1770 and entertained some of the vitriol works employees in his rooms. From one, whom he bribed with 4½ guineas, he discovered that lead vessels were used by Roebuck and Garbett. Andrew Brown indicates that the said Neil Macbrayne, whom he describes as 'a true friend to the freedom of the arts', was again active in 1771. According to Brown he took a few workmen to Langlone, Prestonpans, 'and in the course of a summer's bathing the mystery came out in the process'. The stolen information was used by Mat. Machen to erect a small works near Govan coal works.² At first Roebuck and Garbett did not comprehend the purport of these visits, but when it became clear that they were the activities of rivals they applied for the protection of a patent. The need for a patent was evidently imperative, for in 1772 yet another firm took up the manufacture of sulphuric acid. This time it was a London firm, Messrs Kingscote and Walker, who began at Battersea in 1772 with seventy-one chambers, which they continued to operate for a number of years.

Towards the end of 1772 Roebuck and Garbett in terms of their patent brought an action against Messrs William and Andrew Stirling, merchants in Glasgow, who were known to be erecting buildings near Glasgow with the intention of making sulphuric acid in lead chambers. On being challenged, the validity of the patent was called to question by the Stirlings.³

According to the respondents, they carried on 'an extensive manufactory for printing and whitening linen, in the neighbourhood of Glasgow' and 'consumed great quantities of the

¹ *Specifications of Patents and Drawings, 1767-87.*

² *Op. cit.*, vol. II, p. 251.

³ *Session Papers*, F. 166: 18.

acid Spirit or Oil of Vitriol, in that manufacture'. They therefore decided to erect oil of vitriol works of their own. The Stirlings brought forward various arguments and pointed out that another manufactory, in addition to their own, had been erected at great expense in the neighbourhood of the city of Edinburgh by Messrs Steel, Gladstones and Co.¹

The Scottish Courts decided that the patent was bad, on the ground that Roebuck and Garbett had practised the method for twenty years and that in any case it was in general use elsewhere in Great Britain. The case was next taken to the House of Lords, where it was argued (*a*) that the substitution of lead for glass was no new discovery, but only a slight variation; (*b*) that it was no *new* discovery, since it had been in use for twenty years; and (*c*) that the use of lead vessels was known to various people both in England and Scotland. Lastly it was pointed out that while the patent was in favour of Roebuck and Garbett, it was signed by Roebuck only, and therefore should be declared invalid. Witnesses from Bridgnorth and Bewdley were called, one the wife of Sam Falconbridge mentioned above. The Lords upheld the decision of the Scottish Court, declaring that a patent obtained for an invention in Scotland was invalidated by proof of previous use in England.²

Bereft of the protection of a patent, Roebuck and Garbett continued to place what reliance they could on secrecy. When the celebrated French traveller, B. Faujas de St Fond visited Scotland in 1784 he found the Prestonpans vitriol works still surrounded by high walls. All operations were kept secret and no strangers admitted. It was, however, a trying period for both Roebuck and Garbett. Within a decade of the loss of their patent, but not directly attributable to that event, both were overtaken by financial disaster. Roebuck, on account of his greater commitments, was the first to feel the effects of the financial crisis of 1772-3. As a result, Garbett acquired full control of the Steelhouse Lane works in 1773. Then in 1776 he also acquired Prestonpans. What exactly was happening at this period is still obscure, but it looks as if Roebuck and

¹ *Session Papers*, F. 31:20 and F. 166:18.

² *Session Papers*, F. 31:20; *Journals of the House of Lords* (1774), vol. XXXIV, pp. 76, 217.

Garbett were not pulling together, as the following letter shows. Garbett is writing to his son-in-law.

I have made a contract with K(ingscote) and W(alker)¹ and Coney and Gascoigne, for seven thousand bottles of oil of vitriol annually; and we are not to sell to any body else (not even in Scotland) after Christmas next, when the whole sales are to commence on their account at $3\frac{1}{2}d.$ per lb, and six months credit as usual, for twenty-one years, expirable on two years' notice. I don't let Doctor Roebuck know this until I come to Scotland.²

At Birmingham, Garbett took in a partner in 1776 and the firm became Samuel Garbett and Co. In 1782 Prestonpans was sold to Pat Downey for £11,000, and the following year, on Garbett's failure, Steelhouse Lane passed to James Alston, under whose name it continued for many years. Both Downey and Alston were connected with their respective works for a number of years before becoming sole proprietors. Garbett later recovered sufficiently to start again at Birmingham Heath as an independent manufacturer.³

Change in ownership and reversal of private fortune did not react adversely on the prosperity of the Prestonpans vitriol works. When Faujas de St Fond visited Roebuck in 1784 the works was still the greatest manufactory of sulphuric acid in Great Britain. Sir John Sinclair spoke of it in the following terms:

A manufactory of oil of vitriol, *aqua fortis*, and spirit of salt is carried on here. . . . of late it has extended to white ashes and Glauber salts. More than fifty men are employed, some by day and others by night. They are bound under indentures for twenty-one years, during which time they are paid weekly 6s. for stated wages, with a proportional allowance for extra work.⁴

¹ Garbett simply gives the initials K and W. We presume this was the firm of Kingscote and Walker mentioned above and founded in 1772.

² S. Garbett to C. Gascoigne, 2 November 1776, cited in Bebbington, p. 16.

³ R. B. Prosser, *Birmingham Inventors and Inventions* (1881), p. 16; P. S. Bebbington, *op. cit.*; *Session Papers*, F. 29:31.

⁴ *Statistical Account of Scotland* (1791-9), vol. XVII, p. 67.

Then follow details of raw materials consumed, prices of products, etc. Sulphur came from Leghorn, nitre from the East India Co.'s sales in London, while sixty tons of local coal were consumed per week. Oil of vitriol sold at $3\frac{1}{2}d.$ per lb, *aqua fortis* at $7\frac{1}{2}d.$ to $10d.$, spirit of salt at $6d.$, Glauber's salt at $12s.$ per cwt, and white ashes at £1. 8s. per cwt.

Following the failure of the partners in the Birmingham and Prestonpans vitriol works, expansion in the industry was rapid. As already mentioned, Garbett himself started again at Birmingham Heath, and in 1778 Thomas Farmer and Co., which continued to operate for over a century, was founded. In 1783 a nephew of Walker of Kingscote and Walker founded an offshoot of that concern, namely, Walker, Baker and Singleton, at Pitworth Moor near Manchester in Lancashire. Thereafter the pace of development is so rapid that detailed data for all individual firms cannot be given here.

To a second eighteenth-century revolution in the art of bleaching we attribute a further impetus to expansion in sulphuric acid manufacture. Increased *tempo* in textile production put considerable pressure on cloth finishers to accelerate their processes. Improvement was first effected by the substitution of sulphuric acid for sour milk, followed in 1787 by the introduction of an entirely new bleaching reagent, viz. chlorine. Chlorine required sulphuric acid to make it, so sulphuric acid production kept in step with the expanding textile industry. Further new vitriol works were opened in Scotland. That 'allowed to be the second of the kind in Scotland' and, therefore, next to Prestonpans, was at Burntisland. It was founded c. 1785 by William Muir, who later went to Leith, leaving Alexander Pitcairn and other partners to carry on. By Candlemas 1790, Pitcairn was in sole possession, the other partners having died or withdrawn their shares. This works was extensive, being reputed to contain three hundred and sixty chambers. The cess (tax) paid by Pitcairn alone amounted to one twenty-fourth of the cess paid by the whole town.¹

By 1797 there were six to eight factories in the neighbourhood of Glasgow alone, probably those mentioned as at Woodside, Napier's Hall, Port Dundas, and Carntyne. Further

¹ *Session Papers*, F. 183:30; J. Sinclair, *General Report*, Appendix 2, p. 307.

expansion took place at the beginning of the nineteenth century.

The very intimate connexion between sulphuric acid and bleaching made progressive bleachers realize that they could with advantage make their own chemicals. The first to do so were Messrs Bealy of Radcliffe, Manchester, who erected six chambers in 1799. Bleaching reagents were still somewhat unruly servants, and rendering a bleaching agent transportable brought great fame and a considerable fortune to Messrs Tennant, Knox and Co. of St Rollox Chemical Works, Glasgow. Charles Tennant was originally a bleacher at Darnley near Glasgow. In his name the firm registered two important patents for bleaching with chlorine. Their dry, and therefore readily transportable, bleaching powder was patented in the name of Charles Tennant in 1798. To work the patent, ground for a factory was acquired at St Rollox, Glasgow, where in 1814 some twelve to twenty men were employed, and from this beginning one of the greatest chemical works in the mid-industrial-revolution period developed.¹ It covered no less than ten acres by 1830, with plant engaged in the production of sulphuric acid, bleaching powder, alkalis, soap, etc.

When he first set up on his own, Tennant bought sulphuric acid from the Prestonpans Vitriol Co. and from Messrs Norris and Sons, Halifax. In 1803 he erected his own chambers at a cost of about £50 per chamber. Each chamber burned a thousand pounds of sulphur per week. The rapid progress made by Tennant, Knox and Co. may be attributed to the rapidity with which its promoters adopted any new technological improvement. From the beginning they burned nitre (KNO_3) and sulphur external to the chambers proper, and four years later added a floor to burn sulphur residues which could be bought from less progressive manufactures at the low price of £5 per ton. De la Faille next suggested blowing steam into the chambers instead of using water, and by 1813-14 the innovation was adopted at St Rollox. Annual production of sulphuric acid was then in the region of three thousand tons.

Tennant in turn passed plans for sulphuric acid plant on to

¹ *Ibid.*, p. 313.

Messrs Doubleday and Easterby, Newcastle, who erected the first lead chambers at Bill Quay on the Tyne. The same firm, at a cost of £700, erected the first platinum retort for rectifying acid. This was probably similar to a vessel referred to by Parkes in the *Chemical Catechism*:¹

Some years ago I saw a vessel of platina constructed for the purpose of rectifying sulphuric acid. It holds thirty-two gallons, and cost several hundred pounds; but the advantages which result from its employment are fully adequate to the expense.

Despite the large capital required for this single unit, Doubleday and Easterby soon added two further retorts.²

The cheapness of the abundant supply of sulphuric acid forthcoming began to lessen the cost of erecting lead chambers themselves, since hydrogen generated from sulphuric acid became correspondingly cheap, and was used by lead burners in jointing lead sheets from which chambers were made.

By 1820, in addition to the factories in Scotland which have been mentioned, there were twenty-four in England, distributed according to James Mactear, as follows: London 7, Staffordshire 2, Bristol 2, Birmingham 4, Leeds 1, Halifax 1, Rotherham 1, Newcastle 1, Bolton 2, Manchester 2, Whitehaven 1. The importance of Birmingham is noteworthy.³ The rapid expansion which these figures illustrate is evidence of continually increasing pressure for larger and larger quantities of sulphuric acid, whose commercialization was in turn leading to the foundation of a more involved chemical industry.

Technological improvements brought about a gradual reduction in the price of sulphuric acid till the selling price remained fairly constant at $3\frac{1}{2}d.$ per lb, or £33 per ton. Raw material costs varied somewhat in accordance with local conditions: sulphur from £7 to £16 per ton, nitre from £36 to £64 per ton. Although the ratio of nitre to sulphur used was small, being of the order of one to ten, its high price had a

¹ P. 371 n.

² T. Richardson, J. C. Stevenson, and R. Chapman, 'On the Chemical Manufactures of the Northern Districts', *British Association Report* (1863), p. 701.

³ James Mactear, 'History of the Technology of Sulphuric Acid', *Proceedings of the Philosophical Society* (Glasgow, 1881), vol. XIII, p. 409.

marked effect upon the total production cost, which worked out on an average at $2\frac{1}{2}d.$ per lb or £22 per ton. Sulphuric acid manufacture, therefore, had an important influence on the production cost of bleached and printed cotton goods, on soap, glass, and alkalis, and, as Great Britain exchanged such commodities for colonial products like raw cotton, silk, indigo, etc., its fundamental position in early nineteenth-century economy cannot be overrated.

Frequent reference to the use of oil of vitriol by the bleaching industry at various stages of its evolution must not be taken to suggest that this was the only important influence that it had on the course of industrial evolution. Yet another industry, or perhaps trade, was revolutionized by Roebuck's pioneer works at Birmingham and Prestonpans. Successful commercialization of a process for the conversion of common salt (sodium chloride, NaCl) into soda (sodium carbonate, Na_2CO_3) signals what is usually termed the foundation of the heavy chemical industry. This was secondary to, and dependent on, supplies of cheap sulphuric acid which became available during the second half of the eighteenth century in quantities sufficient for commercial operation. By-products from the soda synthesis afforded new raw materials to revolutionize the bleaching industry.

The works of an alkali manufacturer tended to become larger and more complicated; he began to make soda, using common salt and sulphuric acid and other raw materials. After a time he started to make his own sulphuric acid by burning sulphur or pyrites; if he used pyrites it was probably a mixed sulphide of copper and iron, and it was comparatively easy to make copper sulphate and ferrous sulphate from roasted pyrites. The process of making sodium sulphate produced large quantities of hydrochloric acid, and, as nitric acid was required in the manufacture of sulphuric acid, the alkali manufacturer easily developed into a manufacturer of acids. . . . It was very common for the alkali manufacturer to use the chlorine he recovered so as to make bleaching-powder, and in these ways he became a maker of . . . bleaching powder. The manufacture of all these *heavy*

chemicals became in this way an involved process, in which one part was dependent on the others and almost every effort to prevent waste involved the manufacture of some new product.¹

The influence of these developments upon advancing industrial technology in the early nineteenth century was described by the illustrious Liebig in the following words:

The manufacture of soda from common culinary salt may be regarded as the foundation of all our improvements in the domestic arts; and we may take it as affording an excellent illustration of the dependence of the various branches of human industry and commerce upon each other, and their relation to chemistry.²

The need to solve the salt-to-soda problem became increasingly pressing as the Industrial Revolution pattern developed. While earlier manufacturers' demands were met by importation of weed and wood-ashes, barilla (imported seaweed ash), and kelp (Scottish seaweed ash), demand far exceeded supply and for several decades towards the end of the eighteenth century Scottish kelpers enjoyed a boom in their product.

A detailed history of the multitude of attempts to solve the salt-to-soda problem during the latter half of the eighteenth century has yet to be written, and the function of the remainder of this paper, severely limited as it is by lack of space, is to sketch, particularly in relation to the use of sulphuric acid, some of the efforts that were made between the founding of the first sulphuric acid works in Great Britain and the usually accepted foundation of the alkali industry, i.e. the founding of a factory operating the patent of Nicholas Leblanc (1742–1806)³ by James Muspratt (1793–1883) in the neighbourhood of Liverpool in 1823. It will be remarked how the pioneers in

¹ S. Miall, *History of British Chemical Industry* (1931), p. 5.

² *Familiar Letters on Chemistry*, p. 21.

³ When the scarcity of ashes throughout Europe became so acute during the revolutionary wars, the *Académie des Sciences* in 1775 offered a prize of 2,400 *livres* for a method of making alkali from non-vegetable sources. This stimulated Nicholas Leblanc to submit about 1790 the process which now bears his name. His patent is dated 25 September 1791, but he was not awarded the prize. His works were confiscated in 1793, and he died in the workhouse by his own hand.

the vitriol field contributed to the solution of the co-extensive problem.

Roebuck himself while at Prestonpans was probably interested in soda production. Associated with him were his life-long friends and correspondent, Dr Joseph Black (1728-99), Professor of Chemistry in the University of Edinburgh, and James Watt (1736-1819), who later became a partner in the famous Boulton and Watt enterprise at Soho, Birmingham. It has been suggested that this attempted synthesis of soda was the cause of bringing Watt and Roebuck together through the intermediacy of Dr Black, whom Watt had known when they were both associated with Glasgow University. There is no evidence of production on a commercial scale, although T. S. Ashton speaks of Roebuck acquiring interests in coal mines and *soda works* at Bo'ness.¹ The high duty on salt militated against success.

One of Roebuck's and Watt's friends was more successful. He was James Keir (1735-1820) who like Roebuck was an Edinburgh-trained chemist. Keir engaged in glass manufacture (another industry that made large demands on the supply of soda) at Stourbridge from 1775 to 1778 in partnership with John Taylor and Skey of Bewdley, the latter of whom has already been mentioned as a manufacturer of sulphuric acid. Even at this time Keir was evidently interested in the manufacture of soda, for he opposed a patent application from Alexander and George Fordyce which would have secured to them the sole right to that manufacture.² In 1780, Keir joined forces with Alex Blair of Tipton, Staffs, and among the activities in which they engaged was the making of soda and soap, the soda being made from common salt by the use of sulphuric acid.³

The focus of activity once again returned to the Firth of Forth. When B. Faujas de St Fond was visiting Roebuck in 1784 he met Dr Swediaur, a European authority on venereal disease, who told him that he had purchased an estate at

¹ T. S. Ashton, *Iron and Steel in the Industrial Revolution* (1924), p. 50.

² *Journal of the House of Commons* (1778-80), vol. XXXVII, pp. 891, 912, etc.

³ S. Timmins, 'James Keir, FRS', *Transactions Birmingham and Midland Institute*, vol. XXIV (1891), p. 1.

Prestonpans – the site of Dr Roebuck's vitriol works – where he intended to establish a manufactory for salt with the intention of separating the mineral alkali (soda).¹

Dalmuir, near Glasgow, and Newcastle were two districts where the existence of glass and soap works offered a ready market for soda. Newcastle in particular was a large absorber of Scottish kelp (natural soda).

When financial difficulties drove Archibald Cochrane (1749–1831), 9th Earl of Dundonald, from Scotland to stay with friends on the Tyne he found in Newcastle two manufacturers, William Losh and Thomas Doubleday, experimenting independently on the conversion of salt into soda. Doubleday has already been mentioned. Through the agency of Dundonald a chemical works was founded at Walker in 1796, with Dundonald and Losh among the partners, though Doubleday seems to have dropped out. Helen Landell of Newcastle wrote to Matthew Boulton on 26 January 1796, saying that:

Lord Dundonald had lived as a recluse in Newcastle for many months, and has at last exercised his chemical abilities to advantage, and will probably make a large fortune by his substitute for barilla. Our glass manufacturers are contracting with him, and have little doubt of his success.²

The details of Dundonald's patent operated by the works reveal that it depended on sulphuric acid.³

Further developments took place quickly. In 1808 a second Tyneside firm, Messrs Doubleday and Easterby of Bill Quay, began to make soda. It was they who got plans from Tennant, Knox and Co. of St Rollox and built a sulphuric acid plant, the first on the Tyne. They passed the plans on in turn to Messrs Cookson when the latter began to make soda at South Shields. Several other Tyneside firms had been founded before soda manufacture started in Lancashire.⁴

¹ B. Faujas de St Fond, *A Journey through England and Scotland to the Hebrides in 1784* (ed. Geikie, Glasgow, 1907), p. 173.

² H. Landell to M. Boulton, 26 January 1796 (Assay Office Library, Birmingham).

³ Register of the Great Seal of Scotland (1795), vol. XX, nos. 584, 591.

⁴ 'Newcastle: Chemical Manufactures in the District', *British Association Report* (1863), p. 701.

Contemporary developments of considerable interest took place in Scotland, where Dundonald persuaded Lord Dundas to operate his process at an old candle works at Burnfoot of Dalmuir. After much tribulation Dundas achieved success after, we believe, abandoning Dundonald's process. The inventor of the successful process, acting as sole manager for seven years, received a house, £150, and 5 per cent of the profits, under forfeit of £5,000 if he revealed the secret of the process. Profits up to 1809 were reckoned at £18,000, of which £10,000 went to Lord Dundas. The workmen, of whom there were about twenty, were paid 2s. per day, eked out by the provision of house and fuel by the proprietor.¹

Within a few years of the starting of factories at Dalmuir and Walker, soda was being produced in Scotland at Rutherglen Bridge, Port Dundas, and Camlachie, usually in association with sulphuric acid manufacture. In 1816 the market price of the new synthetic soda was £60 per ton, but when one remembers the low soda output of kelp, or for that matter the best imported barilla, the price is understandable.

In Great Britain, Lancashire and Cheshire, with their vast resources of salt, were the natural focus upon which an extensive synthetic soda industry should converge. The founders of the industry in this area were James Muspratt (1793–1855) and Josiah C. Gamble (1776–1848). Both came to the Liverpool area from Ireland. Gamble, however, was educated in Glasgow, where he attended Dr Cleghorn's class in chemistry. During his course he obtained knowledge of the chemical inventions of Messrs Tennant, Knox and Co. of St Rollox. In 1812 Gamble established a bleaching works at Monaghan in Ireland. There he operated the Tennant process for the preparation of bleaching materials, purchasing the necessary oil of vitriol from Tennant, Knox and Co.

In 1822, Muspratt, having been a chemical manufacturer in a small way at Dublin, left Ireland and established himself at Vauxhall Road, Liverpool, in the following year. In 1828 Gamble followed his example, and they joined forces. So was laid the great cornerstone that chemistry contributed to the Industrial Revolution. The alkali manufactory at Liverpool

¹ *Session Papers*, F. 241:25.

flourished exceedingly, but acid by-products poured into the atmosphere and devastated vegetation for miles around, even crossing the watery, windswept estuary of the Mersey. The nuisance was so great that the Liverpool Corporation ordered Muspratt and Gamble to remove their works to a less populous district. They migrated to St Helens, and, after a two-year partnership there, Muspratt moved on to Widnes, leaving Gamble in possession of the St Helens establishment.¹

By this time Tennant, Knox and Co. in Glasgow had also entered the soda manufacture. In 1825 the manufacture of soda ash was begun at St Rollox chemical works, and this works eventually became one of the greatest in Europe. About 1830 the firm opened an office in Liverpool under the name of Tennant, Clow and Co., to distribute their products to the textile manufacturers of Lancashire.²

Returning to Gamble, we find that he carried on at St Helens for some time without a partner. In 1835 he interested the brothers Joseph and James Crosfield, soap-boilers of Warrington, in the purchase of a defunct sulphuric acid works adjacent to his own alkali works and laid the basis of the Gamble and Crosfield association. In the following year Simon Crosfield, a Liverpool tobacco merchant, joined the firm, which then became Gamble and Crosfield. So capital acquired in West Indies trade was applied to establish heavy industry in Lancashire in a way similar to that whereby fortunes made by trade in sugar and tobacco in Glasgow helped to develop Lanark, Renfrew, and Dunbartonshire.

After the alkali industry was fully established, yet another field of application for sulphuric acid was opened up, that of artificial chemical fertilizers. It was to personnel in the alkali industry that Justus von Liebig (1803-73) turned in his attempt to commercialize a 'patent manure'. According to Fenwick Allen:

When Liebig was making his researches and working out his theories in Agricultural Chemistry, and when he thought he had discovered the secret of the refertilization of the soil,

¹ J. Fenwick Allen, *Some Founders of the British Chemical Industry* (1906), p. 43.

² E. W. D. Tennant, *One Hundred and Forty Years of the Tennant Companies* (1937), p. 61.

the principal thing being to restore to the soil, as manure, the inorganic constituents which it was found by analysis of the ashes of the vegetation had been taken out of the soil by the plant, he got James Muspratt to carry out his ideas by manufacturing certain manures. This manufacture was carried on at Newton, about the years 1843-4. In this venture Muspratt was joined by Sir Joshua Walmsley, a gentleman of much ability and enterprise, who had only a few years before been Mayor of Liverpool.¹

Liebig's patent mineral manure was a failure, but simultaneously vitally important researches were being carried out by one of his pupils, J. H. Gilbert (1817-1901) and by J. B. Lawes (1814-1900). Lawes and Gilbert between them, in little more than a decade, worked out sound manuring principles. One of Lawes's neighbours, Lord Dacre, directed his attention to the use of bones, which for some soils proved a valuable manure but were ineffectual for others. In consequence hundreds of experiments were set agoing, some upon field crops, others with pot plants, in which constituents of various kinds were tried as fertilizers. Of all the experiments those based on the suggestion originally thrown out by Liebig, that neutral phosphate of lime (in bone, bone-ash or mineral apatite) be rendered soluble by sulphuric acid and the mixture applied to root crops, had the most striking effects. Results first obtained on a small scale were subjected to more extensive trials round about 1840-1.

Lawes was quick to realize the implication of his scientific research, and took out a patent for the manufacture of superphosphate in 1842. This became an important industry in itself, and today is one of the chief absorbers of sulphuric acid. In 1843, he established a factory for the production of superphosphate near London. In the next year, 1844, manufacture of superphosphate was begun at Blaydon, in the north of England, by Dr Richardson. Among the raw materials used were bones, bone-ash from South Africa and America, as well as refuse animal charcoal from sugar refineries.

To be nearer bleachfields in Forfar and Fife, Charles Tennant

¹ J. Fenwick Allen, *op. cit.*

and Co. of St Rollox erected a small sulphuric acid works at Carnoustie, Forfarshire, output starting in 1836. With the introduction of artificial fertilizers they installed a bone-crushing plant in 1846, and supplied surrounding farms with superphosphate and fertilizers. The works have been modernized, and now form part of Scottish Agricultural Industries. A West of Scotland firm, Alexander Cross and Sons, founded in 1830, also owe their continued development to fertilizer production. In 1872 they removed to Port Dundas. At this date Lawes's factory alone was producing more than 40,000 tons of superphosphate annually.¹

We conclude with a link-up of personnel who throughout a long period of sulphuric acid manufacture initiated parallel developments, viz. the Tennants and James Muspratt. Unwittingly, they together brought sulphuric acid into the sphere of international affairs. The source from which British manufacturers obtained sulphur was Sicily. To assure a supply of the vital raw material, John Tennant of St Rollox and James Muspratt entered upon a joint venture and purchased sulphur mines there. A feeling of jealousy resulted in the Neapolitans, and in 1836 a contract was drawn up between the King of the Two Sicilies and the house of Taix, Aychard and Co. of Marseilles in, it is said, an attempt to stabilize prices, which were fluctuating as a result of speculation. The contract gave a monopoly to Taix, Aychard and Co., who proposed to reduce the annual production of sulphur from 900,000 to 600,000 cantars. An export duty of £4. 7s. was also imposed, in consequence of which the price of sulphur rose from £5. 10s. to £15 per ton. The British government regarded the monopoly as a breach of the Treaty of Commerce and Navigation, signed in London on 16 September 1816, and, in the words of Liebig,

As the price of sulphur has such an important influence on the cost of production of so many manufactured goods we can understand why the English Government should have resolved to resort to war with Naples in order to abolish the sulphur monopoly.

¹ E. W. D. Tennant, *op. cit.*; W. G. Armstrong, *Industrial Resources of Tyne, Wear and Tees* (1864), p. 175; A. McLean, *Local Industries of Glasgow and the West of Scotland* (1901), p. 190.

Thus there very nearly was a sulphur war, but the Neapolitans thought better of their association with Taix, Aychard and Co. and the monopoly.¹ Technologically, the short period of monopoly had a profound effect on the subsequent history of sulphuric acid manufacture, which, however, cannot be gone into here. Production went on expanding unabated.

The demand for sulphuric acid was increasing yearly, and with every improvement in manufacture the price fell, and with every fall in price the sale increased.²

¹ *Documents Concerning the Sulphur Monopoly, consisting of the Parliamentary Inquiry into the conduct of the Foreign Secretary* (1841).

² J. Liebig, *op. cit.*, p. 24.

7 *The Macintoshes and the Origins of the Chemical Industry*

D. W. F. HARDIE

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More than half a century has passed since the death of Ferdinand Hurter. The Leblanc System, on which, with George Lunge, he was a chief authority, has now long since ceased to be the basis of the heavy chemical industry. That we are met here to memorialize the name of Hurter is not merely a normal and fitting homage to that distinguished pupil of R. W. Bunsen but an expression of historical consciousness, an indication that we are not indifferent to the history of our industry. The choice of a historical topic for this lecture requires no other justification.

It is a facile and utterly false assumption that the history of chemical industry is merely that of the application of the discoveries and theories of pure chemistry and physics to technology, and that industry has always waited upon developments in the pure field. Such an assumption is nearer to truth today perhaps than it has ever been before. The chemical industry made its appearance and developed for over fifty years before the growth of pure science added significantly to its fundamental techniques. Indeed, we may recall that Hurter, who began his industrial career in Widnes in 1867, was one of the very first to apply rigorous physical chemical methods to an industrial process. Hurter's studies of the conditions of interaction of gases with liquids and solids¹ in this connexion, and his devising of instruments for the accurate control of large

¹ F. Hurter (1877), *Dinglers Polytechnisches Journal*, pp. 223, 224; (1885), *Journal of the Society of Chemical Industries*, p. 639; (1887), *ibid.*, p. 707; (1889), *ibid.*, p. 861.

volumes of flowing gases was even more the work of a pioneer than his remarkable excursion, with Vero Charles Driffield, into scientific photography. Industrial chemistry took its place in the Industrial Revolution before phlogiston and caloric were discredited or the modern notion of atomic matter was more than speculatively considered. The chemical industrialist of the first half of the nineteenth century used fire with the prehistoric abandon of his cave-dwelling forebears. The theories of Sadi Carnot, and the foundation upon them of the science of thermodynamics by Clapyron, Clausius, and Lord Kelvin, left contemporary industrial practice substantially unchanged. Research, in any modern sense, played no significant part in the chemical industry of this country and America¹ until the last century was far spent.

It is a strange contradiction that our country, which derived so much of its nineteenth century prosperity from the applications of chemistry and metallurgy, should have lagged behind the Continent in the study and teaching of scientific methods. So late as 1868, when Dr Hurter was coming to grips with the problems of the Deacon-Hurter process, bringing to his task the training he had received at Heidelberg, Matthew Arnold reported: 'In nothing do England and the Continent at the present moment more strikingly differ than in the prominence which is now given to the idea of science there, and the neglect in which this idea still lies here . . .'²

Historical understanding must be based on knowledge of origins. It is precisely in the records of its beginnings that the history of chemical industry is peculiarly infested with anecdotes, misunderstandings, and oft-repeated and frequently erroneous simplifications. Textbook writers generally hasten on from their 'historical introductions' to their technical chapters, having lit somewhat dim candles before the dusty busts of Roebuck, Keir, Leblanc, perhaps William Losh, and certainly James Muspratt. A small number of recently published papers, however, gives promise of an ultimate authorita-

¹ US 77th Congress (1941), 'Research - A National Resource. II: Industrial Research.' Document no. 234 (Washington, US Government Printing Office).

² M. Arnold, *Higher Schools and Universities in Germany* (London, 1874).

tive documentation of chemical industrial beginnings.¹ In what follows we shall be concerned with the historical moment of the Eve-like birth of the chemical industry from the textile developments which were the main factor in the Industrial Revolution. To give this period a human countenance and interest it will be described through an outline of the work of two men, a father and son, George and Charles Macintosh. There will, too, be considerable appropriateness in the fact that Charles Macintosh was the inventor of bleaching-powder, a substance to the manufacture of which Ferdinand Hurter made his own signal contribution.²

THE CHEMICAL REVOLUTION

The chemical revolution, like the wider technological revolution of which it was part, did not emerge suddenly. Transformation of the nature of matter into useful forms did not wait upon the factories of the chemical industry: salt, sulphur, lime, alum, sal ammoniac, saltpetre, soda, potash, borax, and tartar, to mention a few, were all substances with long records of utility; again, more than two millennia of hilarious history lay behind the use of a certain beneficent enzyme's action on carbohydrates!

The chemical industry was latent in certain developments which gathered momentum with the advance of the eighteenth century. Potash, in increasing quantities, was coming from the trans-atlantic Colonies, as the vast forests there fell before the axes of the westering pioneers. William Cullen of Edinburgh, in 1762, carried out a large-scale attempt to use the birks and bracken of Rannoch to supply a home source of potash.³ Kelp from the Hebrides was growing in importance as a raw material for the soap-makers, particularly in Glasgow and Liverpool.⁴ James Hutton, the great geologist, during the middle years of

¹ See R. Padley, *University of Birmingham History Journal*, vol. III (1951), pp. 64-78; A. and N. L. Clow, *Economic History Review*, vol. III (1942), pp. 47-58, and vol. XV (1945), pp. 44-55.

² D. W. F. Hardie, *Industrial Chemist and Chemical Manufacturer* (1951), pp. 502-5.

³ A. Kent (ed.), *An Eighteenth-Century Lectureship in Chemistry* (Glasgow University, 1950), p. 63.

⁴ A. and N. L. Clow, *Annals of Science*, vol. 5 (1947), pp. 297-316, and M. Gray, *Economic History Review*, vol. IV (1951), pp. 197-209.

the century was financing his scientific leisure by being sleeping partner in a sal ammoniac works, supplied with soot by the tronmen of Auld Reekie.¹ Also, in the mid-century, Dr John Roebuck had at Birmingham and Prestonpans, by the use of small lead chambers, laid the foundation of the vitriol industry. The discovery of Francis Home, the Edinburgh professor, that vitriol could be used as a 'sour' in the bleaching of linen stimulated the demand for that acid.

Vitriol was the first modern industrial chemical. Its manufacture on a factory scale developed during the second half of the eighteenth century; its production, however, did not reach large tonnages until the chemical industry, as a web of inter-related processes, made its appearance and became geared to the great growth of textile manufacture, which resulted from cotton displacing wool and linen from their long-established pre-eminence.

More than a decade before Nicholas Leblanc patented his process, considerable attention and activity were being devoted to obtaining a synthetic soda from common salt.² With the exception of Keir's, these synthetic processes attained little significance on an industrial plane.³ It was not, as has hitherto been commonly supposed, the tax on salt that deterred early enterprisers in this field of chemical industry.⁴ (The 'natural' alkali, barilla, was equally the subject of heavy tax.) The causes were twofold. Until Leblanc's method had been proved, a wholly satisfactory process for the large-scale production of a *good* artificial soda was not available. Soap-boilers and glass-makers could not use, without tedious purification, the various artificial sodas these early processes produced. Even more important as a deterrent to attempting the manufacture of synthetic soda was the fact that natural soda (i.e. kelp and barilla) was available in sufficient quantity and quality to meet the needs of the new textile manufacture for several decades after King Cotton began the spectacular ascent of his throne. Charles

¹ A. and N. L. Clow, *Nature*, vol. 159 (1947), p. 425.

² Lelievre, Pelletier, d'Arcet, and Giraud, *Annales de Chimie* (1797), pp. 58-156.

³ R. Padley, *op. cit.*

⁴ D. W. F. Hardie, *A History of the Chemical Industry in Widnes* (ICI Ltd, 1950), pp. 14-17, 137.

Macintosh, whose career will presently concern us, had, from 1806 onwards, his own extensive chemical works, but in his alkali department he continued to make soda ash from kelp. He was, as we shall see, fully acquainted with Continental developments and he prophesied the impending introduction of synthetic soda, yet, shrewd industrialist and business-man that he was, he did not assume in the direction of soda synthesis the role of pioneer; he did not attempt to anticipate a demand that lay in the future.

From France, then in a ferment of new ideas, there came, during the later eighteenth century, a philosophic interest in chemistry. The famous Lunar Society of Birmingham was a focus of this interest in England, and the great Joseph Black, who had connexions with the Birmingham group, expounded the new doctrines in Edinburgh. Lord Cockburn, in his *Memorials*,¹ tells us that when he was a young man, his elders in the stately drawing-rooms of eighteenth-century Edinburgh sent him shuddering to bed with their talk of revolution. 'It was a relief', he writes, 'to hear some younger persons talk of the new chemistry which Lavoisier had made fashionable . . .'

It is a commonplace that the instruments which made possible the fundamental technological revolution of the late eighteenth and early nineteenth centuries were the textile inventions of Hargreaves, Arkwright, Crompton, Whitney, and Cartwright. It is not so commonly understood that the revolutionary power of these inventions depended upon a great social experiment in the New World – the introduction of organized praedial slavery on a scale unknown in history. This turning of the wheel backward on one side of the Atlantic sent the wheels speeding forward on the other. The slavery of the plantations of the American South enabled the production of cotton to be increased at a rate that would otherwise have been impossible. An immediate consequence of this disturbance of established economy was an increased demand for dyes, mordants, and, above all, an efficient and rapid bleaching agent. It was to meet those first demands that the first modern chemical industry arose. In this phase George Macintosh, and, much more so, his son Charles, were important pioneers.

¹ H. Cockburn, *Memorials of his Time* (1856).

GEORGE MACINTOSH

George Macintosh was born in 1739 in the parish of Alness on the Cromarty Firth. He was the fourth son of Lachlan Macintosh, tacksman in the farm of Auchenluich. His family was of unmixed Gaelic descent, tracing its particular branch to Badenoch, the country of the notorious 'Wolf' and of the now legendary ospreys of Loch-an-Eilean. Before George Macintosh's generation, the family appears to have made no mark in history, even locally. Their name and fame are unknown in Alness today. George was not the only one of his father's sons to go into a wider world. His brother William, after amassing £60,000 in the West Indies, for a time wandered in the East, finally becoming a Bourbonist conspirator and a British secret agent in Revolutionary France, where he enjoyed the intimate favours of the Scots lady who was first the mistress then the wife of the notorious Talleyrand. William Macintosh incurred in some mysterious fashion the enmity of Napoleon, and met his death as a result of imprisonment expressly at the Emperor's orders.

Nothing is on record of George Macintosh's youth. We first hear of him as a junior clerk in a Glasgow tannery on the banks of the Molindinar, the beautifully-named stream where St Kentigern and St Columba exchanged pastoral staffs. Besides tanning leather, the works where young Macintosh was employed mass-produced shoes for the Colonial American market. Despite the affiliating 'Mac', popularly associated with romantic futility and Gaelic ineptitude in the practical concerns of life, Macintosh soon showed his mettle in industrial Glasgow. Before many years had elapsed he had sufficiently established his circumstances to marry Mary Moore, the daughter of an episcopal clergyman. This Miss Moore was the aunt of the John Moore the particulars of whose burial are known to most schoolboys ('We buried him darkly at dead of night . . .'). On her mother's side Mrs Macintosh was a descendant of the Andersons of Dowhill, a family that played a long and notable part in the history of Glasgow, its achievements including the saving of the Cathedral from the zeal of Knox's 'purifying' vandals, and the establishing of one of the first soap-making

ventures in the city. By his wife, Macintosh had at least seven children, of which the second was Charles, born in 1766.

Nine years after his marriage, which probably took place in 1764, Macintosh was in business on his own account as a large-scale manufacturer of shoes, his five hundred soutars outrivalling the output of the tanwork on the Molindinar. Glasgow shoemaking and leather-goods manufacture for the Colonies was at that time an industry having about four times the annual value of the entire iron trade of the city.¹ In 1776 the Colonial market was suddenly closed by the revolt of the Americas. George Macintosh had to find a new outlet for his industrial capabilities.

Some time in the 1750s a certain George Gordon, from Banffshire, was following in London the trade of coppersmith. Gordon was called to repair the boiler of a firm engaged in the preparation of vegetable dyestuffs. The observant coppersmith was surprised to find that the process employed resembled the dyeing with crotal, which he remembered from the days of his Highland youth. Cuthbert Gordon, a relative of the coppersmith, was a merchant and probably also an apothecary in Leith. On hearing of the London manufacture, he set about attempting to produce an improved dyestuff from various lichens native to Scotland. 'After great study,' to use his own words, 'application, pains and expense, both of time and money, in trying various experiments', he succeeded in producing 'a most beneficial dye'. His new preparation he called 'cudbear', that being a Scots by-form of his own Christian name, Cuthbert. The patent, taken out in the names of both Gordons, is dated 26 October 1758 (BP 727/1758).

From Classical times, and probably earlier, it had been known that numerous lichens, when exposed to ammoniacal substances (e.g. urine) and air, yield a purple colouring matter capable of dyeing wool and silk. Peasants in later times were widely acquainted with its use. Federigo, a fourteenth-century Florentine, derived his fortune and family name of Roccellari from his extensive manufacture of a dye of this kind from the rock moss *Lecanora rocella*. The Virginia merchants of Glasgow,

¹ G. Stewart, *Curiosities of Glasgow Citizenship* (Glasgow, 1881), pp. 67, 68, 72, 88.

probably using Leith cudbear, dyed their duffle coats purple as a mark of their mercantile aristocracy.¹

Gordon's process, if he employed the method described in the specification, does not appear to have been a very inventive contribution to the art, and would certainly not justify his claim that the product 'was totally unknown to mankind'. Perhaps today he would have had to have been satisfied with registration of the name 'cudbear'.

In recent times it has been shown that lichens, from which dyes of the cudbear type may be made, contain orcinol (3·5-dihydroxytoluene), a compound which reacts with ammonia and air to give an amorphous brown powder, entirely similar to the dyestuffs formerly prepared from lichens.

For a number of years the Gordons, in partnership with William Alexander and his son, carried on a small factory at Leith for the manufacture of cudbear. Technically, they appear to have been successful; their weakness, like Dr John Roebuck's at his Prestonpans vitriol works, was on the financial side of the business.² Like Roebuck, too, they sought the financial aid of John Glassford of Glasgow. Glassford was at that time one of the City's most powerful Virginia merchants. It was probably a condition of Glassford's taking an interest in the bankrupt Leith cudbear works that his friend George Macintosh should join him in the undertaking. This Macintosh did, assuming active control of the business. Glassford's role in this, as in the vitriol works, was merely that of financial backer: that flamboyant gambler, merchant, shipowner, and moneylender was not a creator and administrator of industries. Macintosh moved the scene of cudbear manufacture to a site he had purchased in what was then the rural outskirts of Glasgow, but is now the populous district of Dennistoun. The Gordons were retained for a time to superintend the process, which evidently involved a considerable amount of 'know-how'.

The cudbear works Macintosh erected must have been one of the strangest factories in the records of industry. To ensure

¹ G. Stewart, *op. cit.*

² Cuthbert Gordon filed his petition for sequestration in April 1776, having failed, because of the 'diligence of some of his creditors', to recommence the manufacture of cudbear, which it appears had been in abeyance for some months.

secrecy, a ten-foot wall encircled the site; within this were not only the factory buildings but the dwellings of the workers. On the highest part of the ground Macintosh erected his mansion of Dunchattan (i.e. 'The Hill of Macintosh'), which served not only as his residence, but as the administrative building and counting-house. The workers were practically all Gaelic-speaking Highlanders. All were solemnly sworn to secrecy as to what went on within the girdling wall. They lived under almost military discipline, having to answer nightly to a Gaelic roll-call, 'a ceremony that was never neglected'. A strangely isolated community it must have been. Many of its members lived and died within the factory wall, without ever attaining the power of expressing themselves in English.¹

The Dunchattan factory, which began production about 1777, continued to produce cudbear until it closed in 1852, and its remaining Gaels were pensioned off. After George Macintosh's death the works were carried on as George Macintosh & Company, first by his son Charles, then by his grandson George. In its early days the factory treated some 250 tons yearly of Scottish lichens, using urine collected in the City at an annual cost of £800. Later, native Scottish sources of lichen failed, and other various lichens were imported, to a total value of hundreds of thousands of pounds, from Scandinavia and Sardinia. Cudbear, although used as a dye by itself, found its chief outlet for admixture with indigo and other vegetable colours. The change of fashion to drab tones, which in the early nineteenth century quenched the chromatic exuberance of the preceding one, and the cheapening of indigo and the lac dyes had a limiting effect on the demand for cudbear. Cudbear manufacture at Dunchattan ceased only four years before William Perkin put his mauve, the first aniline dyestuff, on the market. Cudbear was manufactured by others, and used for modifying the tones of various synthetic dyes almost to the end of last century.² The litmus of our laboratory test-papers appears to be the last survival of this dyestuff with a history half as long as civilization itself.

¹ G. Stewart, *op. cit.*

² *Journal of the Society of Chemical Industries* (1888), p. 67.

CHARLES MACINTOSH

While George Macintosh was establishing his cudbear industry, his son Charles was being most carefully prepared for a career in industrial chemistry. After studying under William Irvine at Glasgow University, he sat for a time under the great Joseph Black at Edinburgh. Young Charles entered upon his vocation equipped with the best training that the 'pure' chemistry of his day afforded. In order to give his son a grounding in business and mercantile matters, George Macintosh sent him to work for a time in his friend John Glassford's counting-house, through which, it was said, there annually passed business to the value of half a million sterling. It is significant of Macintosh senior's acumen that, while he employed his nephew-in-law, the future victor of Corunna, in his office at the cudbear works, he sent his own son to be trained elsewhere.

While still a trainee in Glassford's counting-house, and barely twenty years of age, Charles Macintosh embarked upon his first chemical industrial venture, his partners being his father and Dr William Couper, a Glasgow physician. Young Macintosh and Couper manufactured sal ammoniac from soot by the method Dr Hutton was using in Edinburgh. Their main object was probably to provide an additional source of the ammonia required by the cudbear factory; the sal ammoniac was also sold to those engaged in the tinning of iron, copper, and brass. The Glasgow sal ammoniac works was carried on for about six years, being closed in 1792 as unprofitable.

As the son of the cudbear manufacturer of Dunchattan, it was perhaps not surprising that Charles Macintosh developed an interest in vegetable dyestuffs. Some of his leisure was spent examining native plants for a possible source of a blue colouring matter to replace indigo. In 1786 Charles was sent to the Continent as probably one of the first chemical industrial commercial travellers in history. His mission was to find customers for Prestonpans vitriol and for cudbear. It is not known how successful he was in pushing these commodities, but his visit to the Continent was by no means fruitless in other respects. For a time he lived in the Champagne region, where he combined study of the French language with continued search for

an indigo substitute, turning his attention to *Iris primula*, a plant indigenous to and abundant in the district. Although, to his delight, he obtained a blue colour with alum mordant, he does not appear to have followed up his results.

From France the young chemical missionary went to Holland. He visited a sugar of lead works, and was amazed to learn that the Dutch were importing their lead, coal, and malt from Britain, to which country they exported their lead acetate. Returned home, Macintosh informed his father of this situation, and, with paternal assistance, he set about finding the remedy. Before very long the Macintoshes were exporting to Holland lead acetate of as satisfactory quality as the product received from that country. In due time, sugar of lead, which was in increasing demand as a textile mordant, became an important export of Glasgow.

In 1789, Charles Macintosh, who had once written that lime was his 'favourite nostrum', started the manufacture of acetate of lime. He put this new commodity on the market as a substitute for the more costly sugar of lead, a substitution that brought down the cost of 'red colour liquor' (the product of the interaction of acetate and alum) from three shillings to less than sixpence a gallon. This notable innovation in the mordanting process was not patented by Charles Macintosh, although it is certain he could have obtained valid protection for it. The use of lime acetate was speedily adopted by others.¹

To what extent the activities of the Macintoshes, father and son, were independent at this period is difficult to discover. Macintosh senior engaged in a number of industrial and other activities, including a remarkable attempt, of fourteen years duration, to introduce cotton spinning and weaving at Spinningdale in Sutherland. George Macintosh had great shrewdness in business, but he did not have either his son's training or inventive capacity in the technical field. After the account which will now be given of the establishing of the British Turkey red industry, he is not of further interest to us from the point of view of chemical industrial history.

By the 1780s cotton was a rapidly increasing import to the Clyde. The techniques of its fabrication were engaging the

¹ *New Statistical Account of Scotland* (Lanark), vol. VI, pp. 165-7.

weavers and spinners, whose experience had hitherto been in the linen and woollen manufacture. The transition phase was concretely represented by the so-called 'blunks' then being made; these were coarse handkerchiefs with linen warps and hand-spun cotton wefts. In 1780 James Monteith of Anderston, one of George Macintosh's intimates, warped the first web of pure cotton woven in Scotland.¹ A few years later, Arkwright, who had been invited to come north by the Clydeside manufacturers, met Macintosh's friend David Dale, a prosperous lawn and cambric merchant, interested in cotton manufacture. Dale persuaded Arkwright to become his partner in the establishing of water-power cotton mills on the Clyde near Lanark (1785). The Dale-Arkwright partnership did not long survive, but the genial and undaunted Dale, said to be the prototype of Scott's Bailie Nicol Jarvie, carried on the enterprise at New Lanark. Eventually, when Dale forsook industry for preaching and philanthropic pursuits, his son-in-law, Robert Owen, carried out in these mills his famous, and, to his contemporaries, dangerous, experiment in benevolent industrialism.

While cotton manufacture was making its rapid expansion on the Clyde and south of the Border, in Lancashire, in the 1780s, very little was known in this country of processes for dyeing the fabric, in particular the use of Turkey red (madder) for that purpose. On this subject David Dale conferred with his friend George Macintosh. At that time the centre of Turkey red dyeing on the Continent was at Rouen. It was from that city that a certain Papillon came to Glasgow as an invited expert to instruct Macintosh and Dale in the complicated mysteries of the madder vat. As proprietor of the secrets of the process, Papillon, in 1783, entered into an agreement, to which Macintosh and Dale were also, presumably, parties, with the Commissioners and Trustees of Manufactures in Scotland, by which, in return for a ten-year monopoly and a financial consideration, the process should be put freely at the public disposal in 1803. Professor Joseph Black received on behalf of the Commissioners an account of the secrets of which Papillon claimed to be the repository. Either from guile or inability, Papillon failed to impart much of industrial importance to the 'illustrious

¹ G. Stewart, *op. cit.*

Nestor' of Edinburgh. Before very long Papillon was dispensed with by Macintosh and Dale. In 1787, George Macintosh wrote to his son Charles: 'Papillon has now left us entirely.¹ We could not manage his unhappy temper. I have made a great improvement in his process. I dye in twenty days what he took twenty-five to do, and the colour better. We paid him his salary up to October, so as to be quite clear of him.' The Turkey red works Macintosh and Dale established in Glasgow was the first in Britain. Their dyeing is said to have been inferior to that of the Continental factories, because of their use of 'inferior alkali' (presumably, Scottish kelp), instead of good barilla.² In 1805, the process having entered the public domain, Macintosh being advanced in years, and Dale having deserted industry for his spiritual calling, the Turkey red works at Dalmarnock was sold to Henry Monteith of Carstairs, who was to equal and even outrival the skill of the Continental madder dyers.

In 1788, after a stormy passage from Sunderland to Rotterdam, Charles Macintosh was again on the Continent. He visited the Leipzig Fair, and later proceeded to Berlin, where he solicited unsuccessfully a contract for the supplying of cudbear for dyeing the blue clothing of the Prussian army. At this period, young Macintosh was probably still very much under his father's tutelage. In 1790, Charles married Mary Fisher, the daughter of a Glasgow merchant, with the blood of James II of Scotland and of the Bruce in her veins. Marriage no doubt freed him from the industrial fortress of Dunchattan, and, before very long, he became the leading partner in an enterprise entirely separate from his father's cudbear, Turkey red, and other interests.

The expansion of cotton manufacture and the development of the art of dyeing cotton fabric greatly increased the demand for alum mordant. With his friends James Knox, John Finlay, John Wilson, and Charles Stirling, Charles Macintosh set about manufacturing alum from aluminous shale at Hurlet, near Paisley. This appears to have been the first alum works in

¹ After leaving Macintosh and Dale, Papillon became manager of a nearby rival Turkey red works. See *Statistical Account of Scotland*, vol. XII, p. 113.

² G. Macintosh, *Biographical Memoirs of the late Charles Macintosh* (1847), appendix 1, p. 121.

Scotland. The shale was a waste material from coal-mining operations in the district, being removed from between the lime and coal strata. At Hurlet the shale excavation was $1\frac{1}{2}$ miles long and three-quarters of a mile wide. Between 1768 and 1782, unsuccessful attempts had been made by others to make alum from this expanse of waste; Charles Macintosh, in 1795-6, 'by proper application of the principles of chemistry', had succeeded. The shale consisted of a mixture of iron sulphide (pyrites), alumina, and silica, together with lime and coal. On exposure to air the pyrites in the shale oxidized slowly to sulphate. By a series of reactions between the sulphate so formed and the alumina, aluminium sulphate ultimately resulted. Potash was added to the lixivium from the oxidized shale. Finally, the solution was evaporated to small bulk and allowed to crystallize. In connexion with this evaporation of the alum solution Macintosh invented a special type of reverberatory furnace, in which the flame swept over the surface of the liquid, and the 'watery parts' were carried off to the chimney. This evaporating furnace was later widely adopted in the chemical industry for the concentration of soda; it seems highly probable that it was the origin of the common practice of the Leblanc manufacturers of using the flame from their black ash furnaces to evaporate the liquor from the Shanks vats.

BLEACHING-POWDER INVENTION

The year 1797 is an important one in the history of the early chemical industry. In that year Charles Tennant,¹ an enterprising young linen bleacher, came to Glasgow to exploit his invention of using a suspension of lime in place of a solution of potash in the chlorine bleaching vat. He had acquired a site for his new operations at St Rollox, and was joined in his venture by Charles Macintosh, Macintosh's alum-making partner Knox, and Couper, who had managed the Macintosh sal ammoniac works. It was while he was a member of this partnership that Macintosh made an inventive contribution to the chemical industry which alone would justify ranking him

¹ E. W. D. Tennant, *A Short Account of the Tennant Companies 1797-1922* (privately printed, 1922), pp. 12, 13, and W. Alexander, *Chemistry and Industry* (1943), p. 411.

among the first half-dozen important pioneers of the chemical phase of the Industrial Revolution.

Powered by the waters of the Clyde, by the streams of Lancashire, and by Watt's improved steam engine, the cotton industry in the 1790s was overtaxing the capacity of the bleachers' greens with the inexorable miles of fabric poured forth. Traditional bleaching required many weeks of tedious exposure of the textile to sunlight and moisture: no acceleration was possible by modification of this process. This slowness of the bleaching stage threatened to set limits to the rate of growth of textile manufacture. After 1790, chlorine had been used for bleaching in this country, but its objectionable character when used as the gas, and the obvious difficulty of generating it at the bleaching sites, had restricted its application. It afforded no significant relief to the congested economy of the textile industry. A new means of applying the halogen was urgently required. On the solution of that chemical problem further expansion of the cotton industry almost solely depended. Charles Tennant had moved part of the way to solving it with his wet lime process (BP 2209/1798), but this still had serious disadvantages which prevented its wholesale adoption. It was Charles Macintosh who solved the problem completely with his process for absorbing chlorine in dry lime – the first gas-solid reaction to be technically exploited. The bleaching-powder process is one of the simplest and certainly one of the most momentous inventions in the history of industrial chemistry.

Because the bleaching-powder patent (BP 2312/1799) bears only the name of Charles Tennant as the true and first inventor, it has been widely accepted that this was indeed the case. Contemporary evidence is to the contrary. In a letter, dated 30 June 1831, Macintosh is quite explicit in stating his claim to be the inventor: 'You are aware of the extensive nature of Mr Tennant's manufactory of dry chloride of lime (at first established by myself, and *who invented the substance*) . . .' Again, among Macintosh's papers there was found, after his death, a draft of the patent specification in his own handwriting, with amendments inserted by his friend and former fellow-student, Major John Finlay, with whom he was in the habit of discussing his scientific work. It was the view of Macintosh's son (in

his *Biographical Memoir* on his father) that the bleaching-powder patent was taken out in Tennant's name only in order to avoid collision with the earlier wet lime patent (i.e. BP 2209/1798), of which the St Rollox company was also the proprietor. Writers in Macintosh's lifetime gave him the unshared credit of his invention.¹

In 1800, while bleaching-powder manufacture was still on the 50-ton a year scale at St Rollox, an obscure crisis occurred in the affairs of the firm.² Each of the partners put in a sealed tender for the purchase of the works. Tennant, with the financial backing of his father-in-law, Wilson of Hurlet, made the highest bid and obtained financial control. Thereafter, although he appears not to have severed relations finally until 1814, Macintosh took no notable part in the St Rollox enterprise. The exploitation of bleaching powder ultimately brought Tennant world fame and great fortune. There is no evidence that Macintosh was disgruntled with the fact that others had reaped where he had sown, or that there was any estrangement between him and the fortunate Tennant. Macintosh's son, however, seems to have felt that fate played foully by his father, and calculated that, between 1799 and 1846, the use of bleaching powder had effected a total financial saving in the linen and cotton industries of £423,667,014 2s. 1d.!

Whatever the actual dimensions of the saving directly derived from Charles Macintosh's simple invention in the textile industries, it is to be remembered that it also brought about an extremely profitable revolution in the chemical industry itself. Bleaching powder was the key commodity in the nineteenth-century manufacture of chemicals. Vitriol, the original industrial chemical, had its importance first greatly advanced because it was essential to the manufacture of the chlorine required for bleaching powder. In the history of the first seventy-five years of the chemical industry it is almost impossible to overemphasize the significance of chloride of lime. Once the acceleration of bleaching opened the flood-gates of textile manufacture, the demand for soda became so great that it could only be met by its large-scale synthesis from salt.

¹ *New Statistical Account of Scotland* (Lanark).

² E. W. D. Tennant, *op. cit.*

The key role of bleaching powder in maintaining the tottering economy of the Leblanc System in the face of the Solvay process, in the last decades of the nineteenth century, must also be remembered.¹

It was impossible, of course, in 1799, that the St Rollox partners could have any conception of the epoch-making nature of the invention to which Charles Tennant put his name as a mere matter of prudent expediency. Indeed, more than a decade later, they were attempting to obtain through political channels a financial return for the invention. They prevailed upon the Glasgow Chamber of Commerce (of which Tennant was a member) to petition the Government to purchase the invention outright for disposal to the public. A direct approach was also made to the party in power in London, where the efforts of Sir Robert Peel (himself an interested party in the textile industry) were exerted on their behalf. Fortunately, as it turned out, the men of St Rollox were condemned to earn their reward by their unaided private enterprise!

An action by Tennant in Chancery, in 1800, against eleven Lancashire bleachers for infringement of the bleaching-powder patent was dismissed on the ground that the defendants had shown chlorine to have been fixed by means of lime and used for bleaching by a certain Robert Roper as early as 1791-2. Like all epochal inventions, bleaching powder had its crop of claimants to anticipation or, at least, simultaneous discovery. The history of chloride of lime in the earlier stages of its impact on the old bleaching industry has yet to be written.² That the Lancashire men, cited in Tennant's action, were able successfully to plead prior use of lime in connexion with chlorine bleaching does not necessarily imply any precise anticipation of Macintosh's invention. Certainly, the scale on which these English bleachers were working could not, before 1800, have been of much significance; at most their use of lime for fixing their chlorine must have been restricted to rough-and-ready production of small batches of bleach for immediate use. It remained to the St Rollox company to make bleaching powder

¹ D. W. F. Hardie, *A History of the Chemical Industry in Widnes*.

² This is no longer true: see Musson and Robinson, *Science and Technology*, chap. VIII.

manufacture part of the chemical industry and to supply it as a commodity to the textile manufacturers.

George Macintosh, the cudbear-maker of Dunchattan, died in 1807 in an inn at Moffat, in the south of Scotland. He was overtaken by sudden illness while returning from a visit to London. As chairman of the Glasgow Chamber of Commerce he had been on a mission to the southern capital to obtain redress of certain grievances which were then afflicting the trade of the Clyde, among these being the 'shamefully unequal' reliefs on the rock salt duty in the two kingdoms. This apparent breach of Article VIII of the 1707 Treaty of Union¹ was causing 'prejudice and loss to the Scotch manufacturers employed in making the much prized bleaching powder'.² Thus, in his last hours, was George Macintosh engaged in the affairs of the infant chemical industry, and the changes which his son's activity had brought about in that field.

In 1805-6, Charles Macintosh and his partners extended their alum manufacturing to a new site at Campsie in Stirlingshire. At Campsie a stratum of aluminous shale, from 1½ to 2 feet thick, provided abundant raw material. In this new factory Macintosh did not confine his attention merely to alum manufacture, but set about making a number of the chief chemicals of that phase of the chemical industry. Annually, some 14,000 tons of coal were obtained in the course of mining the aluminous schist, as well as 450 chalders of lime. Part of the coal was used as fuel in the factory, and the remainder sold to domestic consumers in the surrounding country.³

At his Campsie works Macintosh made soda ash for the bleachers, his raw material being kelp, from which he also extracted the potassium chloride (muriate) required for his alum and prussiate manufactures. Copperas (iron sulphate) and iron ore were additional products of his new activities. Having iron sulphate available, Macintosh employed it in the making of Prussian blue, a pigment accidentally discovered by Diesbach of Berlin early in the eighteenth century.⁴ The

¹ G. S. Pryde, *The Treaty of Union of Scotland and England, 1707* (1951), pp. 86-9.

² G. Stewart, *op. cit.*

³ *New Statistical Account of Scotland* (Campsie), p. 256.

⁴ Frankland, *Journal of the Society of Chemical Industries* (London, 1915), p. 307.

method used at Campsie for making this valuable blue was the already known one of calcining potash with a mixture of shredded hides, woollen rags, and animal horns in iron pots. The product of this crude procedure was extracted with water, and the pigment formed by addition of iron sulphate to the solution. Macintosh – and in this he was an industrial innovator – also manufactured potassium prussiate (ferrocyanide) in quantity by crystallizing the extract liquor from the original calcination. Before that time, potassium prussiate had sold at 6s. an ounce; Macintosh was able to sell it to the calico printers at 2s. 6d. a pound. He was also the inventor of the process of calico printing with Prussian blue, whereby the pigment was formed *in situ* on the fabric by dipping the textile, mordanted with iron sulphate, in a solution of potassium ferrocyanide. For many years Macintosh was the only manufacturer of prussiate in Britain. (In passing, it may be recalled that it was as a manufacturer of prussiates, with his partner Abbot at 14, Parkgate Street, Dublin, about a decade later than Macintosh at Campsie, that James Muspratt began his career as a chemical industrialist.) It is of incidental interest that it was in the course of his operations at Campsie that Macintosh devised the simplified form of the Baumé hydrometer, wrongly attributed to Twaddell, the Glasgow instrument-maker who first made it to Macintosh's specification.

After his father's death the control of the Dunchattan cudbear works was in Charles Macintosh's hands. In 1819, the year following the opening of Glasgow gasworks, Macintosh entered into a contract to receive quantities of tar from that source. This tar he distilled, and obtained, not only the ammonia required for the cudbear works, but considerable amounts of 'coal oil' (naphtha). Faced with the problem of finding a profitable use for this crude naphtha, he began to experiment with rubber solutions made with it. Its solvent action on caoutchouc was already a well-known property of naphtha. Macintosh, who was extremely knowledgeable on most contemporary technical matters, may have read the Edinburgh medical student (later the famous surgeon) James Syme's proposal to waterproof fabrics by treating them with

naphtha solutions of rubber.¹ With his knowledge of scientific affairs on the Continent, Macintosh may also have known that Professor J. A. C. Charles, in 1783, had ascended from the gardens of the Tuileries in Paris by means of a silk balloon rendered gasproof with caoutchouc varnish. Again, the fact that most of the chemical products of his time found their immediate or ultimate application in the textile industry may have suggested to Macintosh such a utility for his rubber solution.

If Macintosh had gone no further than to coat fabrics with rubber solution, the immortality of his name in the English language would have been lost to him. Fabrics treated with the solution, in the manner suggested by Syme, were unpleasantly tacky and quite unsuited for making articles of wearing apparel. This very adhesiveness was what Macintosh made use of. His invention had again the striking simplicity of his bleaching-powder process. In his 1823 patent (BP 4804/1823) for water- and air-proof fabrics, Macintosh claimed:

A manufacture of two or more pieces of linen, woollen, cotton, silk, leather, or paper, or other the like substances . . . cemented together by means of a flexible cement, the nature of which said manufacture is that it is impervious to water and air.

The flexible cement was, of course, the naphtha solution of rubber, which he made by dissolving ten to twelve ounces of shredded caoutchouc in a wine gallon of 'coal oil'. The adhesion of the treated fabrics to one another was assisted by application of pressure, and the naphtha was evaporated by exposing the united materials to a temperature of about 140° F.

After making his waterproofs for a time in Glasgow, Macintosh opened, in association with the Birleys, a new factory in Manchester, continuing to make the naphtha solution only at his original works in Scotland. In the 1820s railways still lay a short distance in the future. Travellers on horseback and on

¹ J. Syme, *Annals of Philosophy* (1818). Dr H. Sherer in his excellent paper delivered before the Newcomen Society on 13 February 1952, 'The Mackintosh: the Paternity of an Invention', gives a fully documented account of the pre-Macintosh development of the rubberizing of textiles.

the outsides of stage-coaches had to face the rigours of long, weary journeys in wind and rain; to them Macintosh's 'life-preservers', as they were called, came, like the well-known pen nibs, as a boon and a blessing. When Captain John Franklin, in 1824, set off on one of his explorations he took with him bags of Macintosh waterproof, which could be inflated for his men to sleep on – surely the first men in history to sleep on air! Three years later, the stores of Captain William Edward Parry's North Pole expedition were wrapped in Macintosh's patent fabric. Opposition to the 'life-preservers' came from two quarters: the tailors and the doctors. The opposition of the former caused Macintosh to set up his own selling agency, and it was in that way that his name came before his grateful public. The doctors' opposition was based on the overt ground that the garments prevented healthful perspiration; Macintosh's son attributed it to their realization that the new cloaks were 'a decided enemy to their best friends, colds and catarrhs'!

THE ORIGIN OF AN INDUSTRY

Fittingly, in 1823, the year of his second great invention, Charles Macintosh was elected a fellow of the Royal Society, a distinction more frequently then than now conferred upon men whose scientific achievements lie outside the pure and academic field.

We now come to a remarkable example of the play of chance in the origin of an industry. During the third decade of the last century a certain cultured widow, Madame Daubrée, was the proprietress of a very select *pension* in Paris. In her rooms Hector Berlioz gave instruction in music, and the romantic painter, Ary Scheffer, taught painting, incidentally finding time for a beautiful portrait of Madame. The Daubrée *pension* was much frequented by British visitors to Paris. Madame Daubrée's son, Edouard, met there a '*charmante écossaise*', Elizabeth Pugh Parker, who soon became his wife. This Edouard Doubrée and his cousin, Aristid Barbier, were not-very-prosperous makers of agricultural implements at Clermont. In 1832 their business fell on evil days. It was then that Daubrée's young wife, the former Miss Parker, remembered how she had seen her uncle, Charles Macintosh, make balls

of rubber to amuse her and the other children. She proceeded forthwith to make similar balls in a corner of her husband's factory. The demand for these toys astonished her husband and his cousin; they put her in charge of a number of workers engaged in this novel manufacture. In time, rubber fabrication at Clermont was extended to a variety of articles of utilitarian nature – tubes, belts, and valves. In the due course of history this enterprise at Clermont became the great Michelin concern of today. The writer of the unpublished history of that company¹ has well described the industry as the '*enfant de la balle*' – the ball that Charles Macintosh, in a playful hour, made for a favourite niece.

It may reasonably be assumed that Macintosh's interest in ferrous metallurgy in the early 1820s derived from two circumstances. He was mining a considerable amount of ironstone at his Campsie workings, and among his friends of this period was James Beaumont Neilson, manager of Glasgow Gas-Light Company. Neilson, who had made the city's gasworks 'the most perfect and beautiful establishment of the kind in the kingdom',² was, no doubt, by this time giving some thought to his own invention which was to revolutionize entirely the iron industry of this country.

In 1825, at his country home of Crossbasket, Lanark, Macintosh carried out extempore experiments in steel-making. Simple and gratifyingly successful experiments they were. Using an old gun barrel as his furnace and gas from an oil gas-lighting plant, Macintosh passed a stream of 'carburetted hydrogen' over heated rods of iron; within a few hours, instead of the days required by the cementation process, the conversion to steel was complete. Although, with the assistance and advice of his friend Dr Wollaston, FRS, he patented this invention (BP 5173/1825), Macintosh neither exploited it himself on any significant scale, nor succeeded in persuading others to do so. His son gives the somewhat vague explanation that the technical difficulties of constructing suitable furnaces were too great.

Macintosh's steel invention had an incidental and interesting

¹ Michelin Company, *De la Balle au Pneu Velo*. An unpublished history of the Michelin Company (Clermont-Ferrand, France, Robert Puiseux et Cie., 1951).

² *New Statistical Account of Scotland* (Lanark), vol. VI.

side-issue. Dr Hugh Colquhoun, repeating the process experimentally, observed that there were occasionally formed small amounts of 'metallic carbon of great hardness'¹ – this is the first recorded synthesis of graphite.

In 1828, three years after these steel-making experiments, Macintosh sponsored his friend Neilson's 'hot blast' invention (BP 5701/1828). Neilson had succeeded, where John Wilkinson the great English ironfounder had failed, in devising a means of supplying a hot air blast to iron furnaces. The importance of Neilson's invention was that it enabled the Scottish hard splint coal to be used in blast furnaces without previous conversion to coke, and effected a two-thirds reduction in fuel consumption. Macintosh, Neilson, and two others formed a partnership to develop the use of the 'hot blast', which was also worked under licence agreements by other ironmasters. In four decades the iron output of Scotland was increased from some tens of thousands to a million tons a year. Macintosh's great industrial ability was at the disposal of Neilson, and although his name is no longer associated with it, there appears to be little doubt that Charles Macintosh was the foster-father of this great development in iron technology.

OTHER CONTRIBUTIONS TO TECHNOLOGY

Charles Macintosh's principal technical interests have now been outlined; they by no means represent the sum of the results of his Leonardine curiosity and the application of his intensely capable and inventive mind to the technical problems of his time. It has already been shown that he was the first to bring coal tar, on a large scale, into the ambit of industry, if we except Lord Dundonald's activities in connexion with coal distillation at Culross in Fife, in 1782.² Macintosh's use of coal tar as a source of ammonia and naphtha did not, however, afford the relief the infant gas industry required from its embarrassing by-product. Coal tar was the subject of one of those peculiar reversals of technological importance, by which the by-product of a process becomes more important than the product for which it was originally carried on. Lord Dundonald had made

¹ H. Colquhoun, *Annals of Philosophy* (1826).

² A. and N. L. Clow, *Economic History Review*, vol. XIII (1942), pp. 47–58.

tar in considerable quantities as a protecting medium for the hulls of ships; incidentally he made coal gas, but seems to have dismissed it as a passing curiosity.¹ In the 1820s the gas industry was fouling the waters of the Clyde, Forth, and Thames with its rejected tar. A solution of the problem was imperative if the industry were to continue its expansion. With characteristic simplicity of approach, Macintosh devised a means of using the tar as a furnace fuel. No patent was taken out for this invention, which was first put into use at Manchester gasworks, of which one of Macintosh's partners in waterproof manufacture was a director. Tar was for a time widely used in this manner, and a saving of millions of pounds is said to have resulted from the expedient.

A number of examples of Macintosh's ingenuity and his technical activity in the public interest may now be given. Macintosh's brother, John, was for a number of years a captain in the maritime service of the East India Company. It may be assumed that it was this connexion that led Macintosh to put certain proposals before the directors of that company for the improvement of their affairs. Among these helpful suggestions was one for casting nitre into hexagonal ingots to save shipping space and to replace iron ballast, and another for the concentration of lime juice (crude citric acid), also to save space, and to prevent fermentation in transit. The first of these suggestions was rejected on the ground – so Macintosh was confidentially informed – that the company wanted not to decrease, but to *increase* the volume of the goods it shipped, so as to increase the patronage which the allocation of transport contracts involved – a curious sidelight on mercantile economics! The East India Company also rejected Macintosh's suggestion that they should mix copper salts and arsenic with tar to prevent fouling of their ships, and thus save the cost of the copper sheets with which they sheathed their hulls. This notion of preventing ship fouling by means of metallic poisons is a remarkable anticipation of present-day anti-fouling paints.

Macintosh put forward a method of making forger-proof banknotes. These notes were to consist of a 'sandwich' of Turkey red dyed cotton between two sheets of paper, the red

¹ Ibid.

cotton being 'watermarked' by means of bleaching powder. If the use of smalt for colouring banknotes and government stamps had not been previously introduced, it is not improbable that Macintosh's triple paper would have found official favour, and, who knows, banknotes and not waterproofs, might now have been known as 'Mackintoshes'!

Remembering, perhaps, what he had learned on his Continental visits of Guyton de Morveau's application of chlorine as a disinfectant at the time of the French Revolution, Macintosh at various times carried on an unavailing propaganda for its similar use in this country and in the Colonies, in particular for the protection of troops against fevers. The Army Medical Board informed Macintosh that it doubted whether 'the aqueous solution (of chlorine) could be safely regulated by the common soldier or sergeant. . . .'

In 1809, there was great public dissatisfaction in London at the unpleasant taste of bread made with brewers' yeast. The master bakers approached Macintosh and asked him to supply yeast free from 'beery' flavour. A factory was erected in the capital at a cost of £5,000, and Macintosh's brother, Captain John, put in charge. The brewers countered this attack on their yeast monopoly by throwing open their cellars to the journey-men bakers on the understanding that they should force their employers to return to the use of 'beery' yeast. The brewers' ruse was entirely successful; the masters capitulated to the pressure brought to bear on them by their bibulous workmen; Macintosh's London yeast works closed its doors.

Enough has now been said to establish beyond cavil Macintosh's position as one of the important pioneers of the first phase of chemical industry, that in which more or less integrated systems of processes were carried on in factories for the continuous supply in quantity of chemicals as commodities to the market, a market created chiefly by the great development of cotton manufacture. Macintosh's lifetime extended well into the second phase of the chemical revolution, the phase dominated by Tennant, Muspratt, Gamble, and the rest. He took no part in it. Indifferent health afflicted him to some extent during his later years and caused him to interest himself in agriculture, which, too, he approached in a scientific manner.

Charles Macintosh was not merely a local pioneer, organizing traditional or domestic techniques on a factory basis to bring them into step with the tempo of the age. His technical and intellectual horizons were much wider. Over a period of more than forty years he paid five visits to the Continent, meeting French scientists, such as Gay Lussac, Thénard, and Vaquelin. He saw for himself the technological and economic changes that stirred revolutionary Europe at the transition from the eighteenth to the nineteenth century. He brought to his native Clyde the influence of French technics and chemical philosophy. We can be in no doubt that his father's example, his pupilage to the great Joseph Black and to William Irvine (himself Black's pupil), and his view of Continental perspectives were the sufficient causes of his achievements and his success; the final cause lay in the need to express that happy combination of intuition and ability to give it concrete effect, a combination which defies further analysis and which is generally called genius.

Of Charles Macintosh's total personality, apart from his technical genius, little can be deduced from the record. In our modern term, Macintosh was an extrovert, a man whose conscious thoughts found immediate issue in action. His son found no diary or autobiographical material among his father's papers. Neither did Macintosh make any extended record of his scientific and industrial activities. No inside story can be given of those flashes of inventive thought that tamed chlorine to the purposes of manufacture, or protected the bodies of succeeding generations from the lash and chill of the wind and rain. In politics, Macintosh was an outspoken Tory and Conservative, when these terms meant something rather different from what they do today. One feature of his character is brought to light by the numerous letters to him (privately printed by his son) – his great capacity for making and retaining friends, and of persuading others to his point of view. A portrait of Macintosh in his years of maturity, painted by John Graham-Gilbert, shows a beardless face with widely-spaced eyes, straight nose, thick brows, a large and somewhat heavy mouth, a forehead not notably high, and hair showing a tendency to curl; the expression is one of calm resolution; there is little of humour in

it; perhaps, like Maclaren, the *Scotsman's* first editor, Charles Macintosh joked 'wi' deeficulty'! Yet there must have been playfulness in the man who made that rubber ball for his niece.

Macintosh died of influenza, on Friday, 25 July 1843, in his father's former mansion of Dunchattan, within the walls of the old cudbear factory. Perhaps in his last hours he listened reminiscently to the soft Gaelic voices that had once answered to his father's nightly roll-call. He left the respectable fortune of £77,000; a comfortable reward, if not a commensurate one, from his contemporary world, which had accorded him some at least of the recognition and honour that were his due. Ironical circumstance has given his misspelled name to an invention that derived incidentally from his proper chemical industrial activities, and, with the injustice of which history can show so many examples, discharged posterity from more informed remembrance of him.

8 Bryan Higgins and his Circle

F. W. GIBBS

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The late Professor T. S. Wheeler was a scholar born, as well as an industrious and lovable character who gave people credit for far more knowledge than they possessed. Our interests coincided in following the activities of chemists in the eighteenth century, chiefly in Great Britain and Ireland. Two of the more problematical figures at that time were Bryan Higgins, MD, and his nephew William Higgins. Both are remarkable for having attacked the leading chemists of their day. Bryan accused Priestley of plagiarism, maintaining that the latter's celebrated work on the gases owed something, to say the least, to Bryan's lectures and demonstrations in London, some of which Priestley and Benjamin Franklin had together attended at the time of the work on oxygen. William, on the other hand, argued that he, rather than John Dalton, should be regarded as the originator of the atomic theory of chemistry – the 'modern' theory as distinct from that of the ancients. Bryan's claims do not stand thorough examination, for Priestley's work on 'different kinds of air' had already earned him the Copley Medal before he heard of Higgins as a lecturer. Priestley squashed these claims in a book called *Philosophical Empiricism* (1775), but Dalton (being a man of a different stamp, as Partington says) did not answer William.

Professor Wheeler hoped that he might be able to follow up his researches on the life of William Higgins with a book on Bryan, and I agreed that this would be worth while, particularly if attention were given to Bryan's position among an interesting group of London scientists and literary men and to his work on topics that appeared to be physical and commercial rather than chemical. (Professor Partington has since given

first-class accounts of the chemical contributions of both Bryan and William,¹ so that it is not necessary to cover the same ground again here.) This is how I became involved, and why Wheeler made me promise to point out aspects of Bryan's work to which more attention could be given.

Bryan Higgins came of an Irish family, several members of which had been medical men. Most of the biographical information we have about him can be traced through Wheeler's papers first published in the Dublin journal, *Studies*.² Like many of his compatriots, Higgins went to the University of Leyden in order to present a thesis for the MD degree. This explains how it was that he entered in October 1765, and graduated in the following month.³ He then settled in London, and his first address, so far as I can discover, was in Chancery Lane. Of this period he wrote:

A just sense of the utility of the Chemical Art, towards promoting Natural Knowledge, prompted me at a very early period of my life, to employ myself privately in the Practice of Chemistry, and to attend with the utmost diligence to the phenomena which occur in the processes of Chemists and divers other artists.

He was thus following in the footsteps of a group of chemists who had done much to stimulate interest in the development of chemistry so that it, in turn, could play a more important part in the growth of manufactures and the technical arts – men like Peter Shaw, William Lewis, and Robert Dossie,⁴ the authors of the chief books on the subject at the time Higgins was a student.

Of this early period there exists a book on icebergs (1766)

¹ J. R. Partington, *A History of Chemistry* (London, Macmillan, 1962), vol. III, chap. 16 and index.

² In their final form these appeared as Part I of T. S. Wheeler and J. R. Partington, *The Life and Work of William Higgins, Chemist, 1763–1825* (Oxford, London etc., Pergamon Press, 1960).

³ R. W. Innes Smith, *English-Speaking Students of Medicine at the University of Leyden* (Edinburgh and London, Oliver and Boyd, 1932), p. 117.

⁴ The work of these men has been described elsewhere, e.g. F. W. Gibbs, *Endeavour*, vol. 12 (1953), no. 48 (Wilson, Shaw and Lewis); *Annals of Science*, vol. 7 (3) (1951), p. 211 (Shaw); vol. 8 (2) (1952), p. 122 (Lewis); vol. 7 (2) (1951), p. 149 (Dossie); *Plat. Metals Rev.*, vol. 7 (2) (1963), p. 66 (Lewis).

and a patent (1767) for an oil-lamp designed to look like a decorative candlestick. Altogether Higgins patented four inventions, the last in 1802, and this indicates the period of his main activities – when he was aged from about thirty to about sixty-five.

Probably when he was in Chancery Lane, Higgins became acquainted with J. Welland of King's Inn, and he afterwards married Welland's daughter and heiress, Jane. In 1772 his address was Orchard Street, Portman Square, and from there he wrote a paper on a chemical topic that was presented to the Royal Society by his physician friend, Richard Brocklesby, and read at two meetings in December 1772, and January 1773. Brocklesby had the MD of Dublin and of Leyden and had also studied at Edinburgh. By 1774 Higgins had moved to the fashionable area of Soho – then part of Middlesex – at 13 Greek Street. Just as physicians tended to congregate on the north side of Oxford Street, so men of science seem to have favoured Soho Square and its neighbourhood. Here was the London home of George Parker, 2nd Earl of Macclesfield, who became President of the Royal Society in 1752, the year in which he was instrumental in changing the calendar. He was the virtual author of the Bill, and it was against him that the cry of the missing eleven days was raised. Greek Street was the address Higgins used for his scientific work from 1774 onwards. He attempted to be self-supporting through his chemical activities, for at various times he made and sold chemical and pharmaceutical preparations (such as the red lead he made for Priestley's work on oxygen), undertook consulting work (such as analyses for the traders in mineral waters), and carried out research on practical topics, such as cements and the manufacture of rum, the work on cements leading to a well-known patent. He also acknowledged help from several patrons, some of whom may be mentioned first.

At that time scientists worked largely on their own and at their own expense and so, like other men of letters, they needed patrons to assist them with special projects, for example in meeting the cost of materials and books and in supporting their publications. Thus the Earl of Macclesfield helped William Lewis in his work on platina, for which he obtained the Copley

Medal. The first Duke of Northumberland was another such patron; he assisted Lewis, Higgins, and Priestley, among others. The Earl of Shelburne (an Irish peer and later first Marquis of Lansdowne) is perhaps better remembered by chemists as Priestley's patron than as Prime Minister of England in 1782. Field-Marshal Conway, another of Higgins's patrons, was formerly MP for County Antrim and became commander-in-chief with a seat in the Cabinet; he was one of the few notable people who retained office under Shelburne, and is remembered for the bridge he designed to cross the Thames at Henley. Sir Joseph Banks, President of the Royal Society for many years, became famous for his hospitality to scientists at his home in Soho Square. Philip, the 2nd Earl Stanhope, and his son Charles, the 3rd Earl, were also known as patrons of science. Several others are mentioned in the writings of Higgins and Priestley. An account of such patronage deserves a book to itself, but relatively little has been written about its contributions, however indirect, to the development of pure science, technology, and the manufactures during the earlier period of the first Industrial Revolution.

Part of the self-imposed duty of these patrons was to invite scientists to discuss questions of the day with men in other walks of life who were likely to be interested in their work. Thus it was at dinner with the Duke of Northumberland that Priestley got the idea for making carbonated water that quickly led to the manufacture of artificial mineral waters, based on chemical analyses of spa waters, such as those made by Higgins; and it was Northumberland who assisted Higgins in a project connected with the glass industry. Shelburne had large estates in Ireland (where at one time he thought Priestley might be settled), a family home in Wiltshire, and a town house in Berkeley Square, where Priestley entertained politicians and men of science, including Higgins and Brocklesby, by demonstrating his latest experiments.

There were also a number of clubs where scientists and literary men could meet for discussions. The nearest of these to Greek Street was Slaughter's, where several of Higgins's acquaintances met and where no doubt he also was to be seen on occasion. The members included Banks, Captain Cook, Dr

Templeman (Secretary of the Society of Arts), John Smeaton (who made Priestley's air-pump), Dr George Fordyce (another teacher of practical chemistry and a consultant), and Richard Levell Edgeworth. Higgins, like Fordyce and Dossie, also gained admittance to Samuel Johnson's circle, and he is mentioned by Boswell. Johnson, of course, was a friend of Lord North's administration and wrote pamphlets in support of its policies; for this reason, among others, the names of Shelburne and Priestley were anathema to him.

Nor should it be forgotten that the meetings of the Royal Society during the London 'season' brought many men from the provinces and that contact between them was much easier and more direct than is sometimes appreciated. The most important group outside London was centred on the informal Lunar Society of Birmingham, famous for the direct stimulus it gave to men like Matthew Boulton, the 'father' of Birmingham; his partner James Watt, and associate Captain James Keir, the chemist and chemical manufacturer; William Withering, the botanist, chemist and physician; Josiah Wedgwood, the pottery manufacturer; and several others. Higgins also had contact with some of them, though not so intimately as did Priestley, who was indebted to them as patrons as well as friends.

The closest of them all to Higgins were Wedgwood and his partner, Thomas Bentley, whom Higgins referred to in his writings as neighbours. The reason for this was that Wedgwood had his London showrooms for many years in Greek Street, where Bentley was resident manager before he moved to Chelsea, and where Wedgwood himself sometimes stayed. Higgins, like Priestley, was given apparatus of glazed and unglazed earthenware for his laboratory, for Wedgwood was the first to try to supply this rather specialized market, and the laboratories of Higgins and Priestley were the chief ones where materials of various compositions were tried out in use. All these men had chemical interests and contributed directly or indirectly to the work of the Royal Society. Among their papers may be mentioned Watt's on the composition of water, Keir's on glass, Withering's on barium compounds (of importance for Wedgwood's 'jasper' ware), and Wedgwood's on pyrometry.

But these are not the only names that might occur in any

book on Higgins and his circle in London. He acquired a certain celebrity through his lectures and courses of practical demonstrations, and through the subscription club he organized for the discussion of scientific questions, which provided one of the more serious occupations for Soho residents and their friends. This side of his work began in 1774 with the first of a series of courses on philosophical (i.e. theoretical), pharmaceutical and technical chemistry. His avowed purpose in running this 'school,' which later led to his being styled 'Professor of Chymistry', was to obtain money to enlarge his laboratory, and in this he was successful.

After giving three such courses he arranged the 'Meetings . . . for the Purpose of Improving Natural Knowledge' which were supported by fifty subscribers. The first meeting was advertised for 13 November 1775, and was preceded by a special practical course for those who were not familiar with the subject.

Higgins said that his intention was accomplished. He already made small amounts of chemicals to supply other chemists, and eventually he redesigned his laboratory so that he could deal with technical preparations on a larger scale, almost a manufacturing scale. One of his operators or assistants, S. F. Gray, made a plan of the laboratory as it was when he worked there, and he reproduced this in his book, *The Operative Chemist*,¹ which is a rich store-house of practical information relating to the eighteenth and early nineteenth centuries. The laboratory was a good-sized room more than thirty feet long – presumably on the ground floor, as his chimneys were forty feet high. There was a central group of furnaces (with nine flues), including three melting furnaces, two reverberatory furnaces, and a group of sandbaths. The fires were blown by a stream of air led in at ceiling level and presumably heated by the flue gases on its way down. Air-ducts led from the fire to the sandbaths and other equipment. In one corner was a large still, and elsewhere a press for the extraction of natural oils, a tank for work on gases, a balance table, and a useful laboratory bench.

¹ S. F. Gray, *The Operative Chemist; being a Practical Display of the Arts and Manufactures which depend upon Chemical Principles* (London, 1828), pp. 72–4.

The chief chemical problems at that time were connected with the function of heat and light in reactions. This had been made clear by Sir John Pringle, President of the Royal Society. When presenting the Copley Medal to Priestley in 1773 he expressed the hope that he would go on from his work on gases to study and explain these other matters. It is therefore not a coincidence, merely, that the meetings arranged by Higgins were concerned largely with phlogiston and the 'elastic fluids' (gases), or that these subjects, which, he said, he was thought to understand better than any other parts of natural philosophy, were those that he decided to publish first. His book was called *A Philosophical Essay on Light* (1776). Those who wish to trace the development of his chemical ideas must turn also to the final chapters of his *Experiments and Observations . . . on a large number of miscellaneous topics* (1786) and the *Minutes of a Society for Philosophical Experiments and Conversations* (1795), which dealt largely with the theory of heat and the antiphlogistic theory of Lavoisier, which even then was not understood clearly by many, apart from experts. In fact, all Bryan's major writings, despite their titles, reveal his ideas on chemistry rather than on any other science.

It is sometimes forgotten that in the 1770s the terms atoms, molecules, and particles were in common use both in Britain and on the Continent. Higgins began his *Essay on Light* by defining *atoms* as the ultimate parts of any mass of matter. A body consisting of two coherent and heterogeneous atoms he called a *molecule*, 'after the example of modern chemists'. Small bodies composed of cohering atoms are 'by common consent' called *particles*. Elements he regarded as matter whose several parts possessed the same properties, and so elements and compounds were still not properly distinguished. To a modern reader it comes as an anticlimax to find him claiming that there are seven elements, namely earth, water, air, acid, alkali, phlogiston, and light. Priestley had already shown that air was 'compounded' and that its constituent parts were dissimilar, having different densities. Higgins did not accept this; he maintained that phlogisticated air (later called nitrogen) had a greater density than air, despite Priestley's finding that it was less dense. Different kinds of matter, Higgins said, contained

different atoms. (Priestley later found examples of different substances which were composed of the same 'elements', and it was a contemplation of such substances that led Dalton to his atomic explanations.)

Higgins then developed his views on the structure of matter and on chemical theory, introducing ideas of polarity and of definite proportions to explain reactions, e.g. between lime and sand. He also distinguished between phlogiston and fire as the difference between a property and a process – inflammability and combustion. Thus properties and substance were still confused.

Like some others of his contemporaries, Higgins was always alert to the possible practical applications of his observations. As an example we may look at his book on calcareous cements (1780).¹ The behaviour of mixtures of lime, sand, and water was studied in his first course of 1774 to illustrate polarity. From this, it seems, arose his interest in the actions of alkalis and sand to give mortars and glass. His applied research began in 1775, when he was impressed by the fact that the cement used by the Romans had lasted in exposed aqueducts for 1,500 years and more.

Remembering the work of Joseph Black, he first examined the decomposition of chalk and limestone by heat, and showed that the greater the amount of fixed air (carbon dioxide) driven off the better the cement made with it. He then showed how this could be applied in common building practice. He next measured the *speed* with which lime absorbs fixed air from the atmosphere. By experiment he found that a large batch of mortar used a little at a time became progressively less efficient, although builders thought this was the way to get the best results, and he explained why they were wrong. By changing the proportions of lime and sand he searched for the best mixture, and recommended one measure of lime to seven of sand, mixed preferably with lime-water.

Later he tried to find an ideal mixture for plastering and

¹ The full title was *Experiments and Observations made with the view of improving the art of composing and applying calcareous cements and of preparing quick-lime: Theory of these arts; and Specification of the Author's cheap and durable cement, for building, incrustation or stuccoing, and artificial stone.*

stucco work, and examined many materials. Most foreign matter spoiled the mortar, but in 1777 he found that a small amount of bone-ash helped it to set, and this formed the basis of his application for a patent, which was granted on 8 January 1779. Already in 1778 he had persuaded James Wyatt, the fashionable architect of Queen Anne Street, and his brother Samuel Wyatt of Berwick Street, Soho, an equally fashionable builder, to get the plasterers to use his mixtures and to judge their merits. By 1780 he was able to list several buildings, mainly private mansions, in which his mixtures had been used. This was the age of imitation in architecture, the age when older, especially classical, styles were followed without originality and when ruined temples were built from scratch. The *Pelican History of Architecture* speaks scathingly of Wyatt's work, but there is no doubt that he designed plenty of buildings in the London area and in many other parts of the country and that Higgins reaped some benefit from the arrangement. Higgins also produced mixtures in imitation of Bath stone and Portland stone which were used as facing materials. His book on this subject was considered to be of sufficient interest to justify an Italian edition.

The importance of the developing art of analysis was made clear in his lectures, for he devoted a separate section to 'vulgar analytical chemistry'. The best example of his work in this field is the series of analyses of well-waters and spa-waters that he made on behalf of John Ellison, a widely-known London supplier.¹ The demand for mineral waters was sufficient for Ellison to make a horse-drawn 'machine' for the delivery of all the common mineral waters on tap. His book containing Higgins's analyses carried the following advertisement:

Mr Ellison respectfully informs the Public, that his Spruce Beer and Mineral Water Machine sets out between Nine and Ten every Monday, Wednesday, and Friday, from White-chapel; and proceeds by the way of Holborn, Red Lion, and Queen's Square, through Bedford, Cavendish, Portman, Grosvenor and Berkley Square, to his Warehouse in St Alban's Street.

¹ *Synopsis of the Medicinal Contents of the most noted mineral waters, . . . analysed by Dr Higgins, at the instance of John Ellison*. Editions of 1780 and 1788 are known.

Ladies and Gentlemen sending their Orders to the Three Kings, Grange-Street, Bloomsbury, or to the King's Head, No. 113 Oxford-street, at the corner of Bolsover-street, at both which Places the Machine regularly stops, will have them immediately executed.

Spruce beer had been made popular by the account of Cook's voyages, for his men had been given it on the coast of Guinea, and this, he thought, had been an important factor in helping to prevent scurvy.

Higgins also dealt with the making of artificial mineral waters in his discussion meetings, but it was not until 1793 that they became available on anything approaching a manufacturing scale from the domestic factory of J. J. Schweppe and his daughter Collette in Margaret Street, Cavendish Square. It was a remunerative business, for in 1797 they made a clear profit of £1,200 from soda-waters and artificial spa-waters. In 1798 they formed a company, and in 1799 they retired, having received £2,200 for 'know-how' and retaining a quarter interest.¹

During the 1780s Higgins carried out a considerable amount of research, principally on alkalis, glass, and a series of topics dealt with in his *Experiments and Observations on Acetous Acid* . . . of 1786. His patent for soda and potash, dated 31 July 1781, was of some chemical interest. As regards soda, he suggested starting with brine or sea-water, which was first concentrated and then mixed in a definite proportion with sulphuric acid to produce salt-cake. This was transferred to a separate furnace and melted with charcoal to produce the sulphide. Twice its weight of lead was then added while it was still fused, and the mineral alkali floated over the 'sulphurated lead'. The two layers were then separated. Instead of lead, one could use iron or tin. This material was to be called 'British barilla' or 'patent mineral alkali'. Perhaps he rushed this, for on the following day a second patent for the same purpose was granted to Alexander Fordyce of Harley Street, who fused Glauber's salt, iron, and charcoal in stated proportions and pounded the

¹ The original Articles of Agreement, dated 14 May 1798, and Indenture of 14 February 1799, are preserved by Schweppes Ltd. For more details, see F. W. Gibbs, *Journal of the Royal Institute of Chemistry*, vol. 86 (1962), p. 9.

product, leaving it in water for twenty-four hours. These patents were numbered 1302 and 1303, respectively, and it is of interest that Nos. 1306, 1320, and 1321 were a series of specifications dealing with improvements to the steam engines of James Watt.

One of Higgins's students was a second William Lewis who had come to this country from Jamaica and was interested in the manufactures of the West Indies, such as sugar and rum. He became secretary to the Society of Rectifying Distillers, and a member of the Linnaean Society in its earlier years. He regarded Higgins as one of the best chemists of his generation and thought his ideas and suggestions had done more for chemistry than the greatest discoveries of the period.¹ It was probably through this connexion that Higgins was elected to visit Jamaica, where he stayed for five years, in order to improve the manufacture of sugar and the process for rum-making. This was an important decision for him to make, as it involved selling up his apparatus and household effects, together with his 'town chariot' and his scientific collections, advertised as a 'Systematic Cabinet of Mineralogy, Metallurgy, Vitrifications etc.'. The sale, which began on 30 July 1796, included 'All the valuable Stock of Materials for Experiments of Products of Analysis, of Pharmaceutical Preparations, and of Instruments, Chemical, Pneumatic, Mechanical, Optical, Electrical, and Technical, in Metal, Glass, and divers other Wares, several thousand flint glass and green Bottles and Vessels. . . .' The appointment that took him to Spanish Town was of sufficient interest for *The Times* of 4 October to mention that he was to have a salary of £1,000 a year from the West India Merchants and was to receive further benefits if he succeeded in his mission which was 'to improve the distillation of Rum from the Sugar Canes, and particularly the offensive taste and smell of that article when newly made. There is every reason to hope that Dr Higgins will be successful.' On 14 December the House of Assembly set up committees for the improvement of Muscovado sugar and rum, and Higgins's work for them resulted in some publications which have become excessively rare. Apparently his work pleased his

¹ *Gentleman's Magazine*, vol. 93 (1823), p. 185.

employers, for *The Times* referred to his return to England on 14 September 1801, 'on leave of absence', adding that a 'very liberal provision has been settled on him'.

The Committee's reports indicate that his work was highly satisfactory, and his salary was raised to £350 a quarter retrospectively. In the sugar process he improved the design of the coppers to ensure greater fuel economy and accelerated the evaporation by pre-heating the liquor; he widened the flues and introduced bracing-bars across a 'terrace' of coppers so that repairs to brickwork could be effected without dismantling the equipment. His improvements in the still-house were said to have removed the offensive smell of new rum completely. 'The distiller, by setting his vats or cisterns by the hydrometer, and the scale adapted to it, acts with certainty and precision; . . . about a seventh or eighth of the sweets will be saved; and by the use of the ley of the stock-hole ashes . . . the spirit will be so rectified as to attain the perfection desired.'

In March 1801, the Committee 'lamented' that Higgins had to leave Jamaica because of the infirm state of his health and granted him a further £1,000 in addition to his salary.¹ Unfortunately, although the Committee sought to popularize the new methods, conservatism prevailed; they were not widely adopted, and most of the planters discontinued them soon after Higgins left.²

According to his biographer, Sullivan, Higgins retired to his estate at Walford, Staffordshire, where he died in 1820, aged 83.³

Had it not been for Higgins's complete break with Soho it is highly probable that more would have been heard of the Society for Philosophical Experiments and Conversations, which he founded in 1794 on the pattern of his discussion group of 1775. His proposals were issued in November 1793,

¹ *Journals of the Assembly of Jamaica*, vol. 9, pp. 551, 621; vol. 10, pp. 496, 576-7.

² L. J. Ragatz, *A Guide for the Study of British Caribbean History, 1763-1834*, p. 299. I am indebted to the Librarian, University of the West Indies, for this and the previous reference.

³ Partington, *op. cit.*, says 1818, and gives his date of birth as 1737 or 1741. Innes Smith, *op. cit.*, gave c. 1732, but Leyden's records had *aet.* 24 in 1765. Following Sullivan, Higgins's dates would be 1736-7 to 1820.

and the Society was instituted in January 1794, when the number of subscribing members reached fifty. Meetings went on throughout the session of Parliament, and several politicians from both Houses attended. The members included Earl Stanhope, Field-Marshal Conway of Soho Square and Brocklesby, as well as others not apparently connected with the scientific world. Higgins called himself 'didactic experimenter', his talks were 'discourses', and he had a Committee of Publication which – as Partington could not resist mentioning – had a Mr Partington among its members. Higgins's lecture assistant was Mr Thomas Young, a relative of Brocklesby's. Topics were restricted to the physical side of chemistry and to the improvement of technical arts, the members having some say in what was done. Most attention was given to the works of Lavoisier and to the ideas of Crawford on heat. Higgins introduced the new chemical nomenclature in these discussions, but some members objected to this and made known 'their desire that it should be adopted no farther than might be requisite to express new subjects and doctrines'. He was thus forced to begin by expounding the new doctrines. In addition to this, experiments were carried out on the gases, and the members also inhaled some of them to note their effects.

The Minutes of the Society were published in the following year, and it seems to have been Higgins's intention to repeat or continue with the programme in the next session. But this was the only volume published, probably in view of his impending visit to Jamaica. By the time he returned the meetings were no longer necessary, for Banks and Rumford had already brought the Royal Institution into being, for the 'promotion, diffusion, and extension of science and useful knowledge'. Perhaps it was not merely coincidence that the Royal Institution also arranged discourses, and that its first 'professors' were the same Thomas Young who had assisted Higgins, and Humphry Davy, noted particularly for his knowledge of the gases and their effects on inhalation. This alone should ensure that Higgins is not forgotten.

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